THE

Journal of the American Medical Association.

EDITED UNDER THE DIRECTION OF THE BOARD OF TRUSTEES.

PUBLISHED WEEKLY.

VOL. XVI. CHICAGO, MAY 9, 1891. No. 19.

ADDRESSES.

THE PRESIDENT'S ADDRESS.

Delivered at the Forty-second Annual Meeting of the American Medical Association, at Washington, D.C., May 5, 1891.

BY WM. T. BRIGGS, M.D.,

OF NASHVILLE, TENN.

PRESIDENT OF THE ASSOCIATION.

Ladies and Gentlemen: My heart swells with pride when I look over this vast assembly of the representatives of a profession, distinguished alike for its antiquity, its scientific attainments, and its usefulness; a profession which forms the true link between science and philanthropy.

You, gentlemen, as members of this Association, have been delegated by your local Associations, from every part of this Grand Republic, from the Kennebec to the Rio Grande, from orange groves and golden sands, from mountains clad in eternal snows, and valleys smiling in perpetual verdure, to represent them on this occasion.

You are here with no selfish motives, nor purposes of self-aggrandizement, nor to excite or perpetuate sectional antipathies.

Indeed, as physicians you should recognize no section; with you there should be no East, no West, no North, no South. You are here solely for the promotion of science and for the good of the human race. You are here to maintain the honor and dignity of the profession and to hold aloft the flag of honorable medicine. You are here to lay your contributions,—the accumulations of your study and observation,—upon a common altar for the common good; to worship at the sacred shrine of medicine and to renew your fealty to the noble profession to which you have devoted your lives and linked your fortunes. You are here to exchange experiences with each other, to separate by discussion the grains of truth from the wordy chaff in which they may be hidden, to renew friendships and to weave more closely the bonds of professional brotherhood.

As physicians you have an almost superhuman mission to fill. Life, the greatest of human blessings, and health, the greatest stimulant to earthly enjoyment, are the issues with which you are called to deal.

The chief objects of your professional work are to preserve the one, and to procure the other. The goal of your ambition and desire is almost at the end of human capacity. It is your province as well as your earnest desire to know all the secrets of organization. You would have the formative crystal and germinal spot made transparent. You would enter the microscopic world and witness the wonders therein revealed. Indeed, you would, if it were possible, search into and unravel the very mysteries of the vital principle.

To this perfect knowledge you aspire.

It is doubtful if man's intellect, great as it is, can ever compass all that he so earnestly desires, yet by constant and faithful work he may approach nearer and nearer to its consummation.

He has before him rich and boundless fields for research, from which the eager and enthusiastic explorer may gather the richest prizes. To these fields the future generation of physicians will be attracted in the hope and belief that as man advances in knowledge and approaches nearer to the understanding of the perfect wisdom, which designed his physical organization and by which he is brought in relation with the world around him, he will be enabled to solve more and more of the difficult problems which have, for ages, baffled and perplexed him, and to elevate the profession of medicine to a position more nearly akin to that accorded to the exact sciences.

We live in an age of progress, and all the arts and sciences are advancing with gigantic strides. With the aid of steam and electricity time and space have been almost obliterated. The most distant parts of the world have been made neighbors. A fact developed or an important discovery made, is flashed in the shortest conceivable time to the most remote parts of the globe and given to the public without money and without price.

The science of medicine has kept pace with, if it has not outstripped all other sciences.

It has been completely revolutionized within our day. The microscope, chemical analysis, clinical observation and vivisection are carrying the medical mind with wonderful velocity in pursuit of knowledge, far beyond, indeed, the most sanguine conceptions of even a third of a century ago.

In every part of the habitable world blessed

with the light of civilization, active, busy members of our profession endowed with high culture and incited by the noblest resolves are enthusiastically engaged in unravelling the mysteries of disease and seeking the means and methods of treatment for the mitigation and relief of suffering and the prolongation of life.

That the full benefit of the labors of American physicians might be attained and utilized, it was essential that the members of the profession scattered over an area of country of almost inconceivable extent, should be brought into associated action,—should be organized into a body by whose annual discussion an exciting, vivifying and healthful influence might be exerted over the length and breadth of the land, until a correct and noble sentiment had been engendered in the bosom of every member of the profession.

Through the brilliant genius and indomitable energy of one who is known as the "Father of the Association," this grand body was formed and has ever been sustained and fostered by his parental solicitude, in which he has been nobly assisted by the cordial coöperation of his brethren. More than a generation has passed since its organization, and many of the master spirits which were present and assisted in the inauguration of the enterprise have joined the silent majority, leaving it as a precious heritage to their successors, who should be actuated by the same spirit which inspired those who had preceded them, and with the same energy and zeal endeavor to manage the high trust with an eye single to the honor and glory of the profession.

A few of those who assisted in its organization in 1847, weighed down with honors and years, still make annual pilgrimages to this Medical Mecca and by their presence and counsel add increased interest to its meetings.

Chiefest among these is Nathan Smith Davis, to whom, more than any other, is due the credit of establishing and perpetuating this National Association—venerable, distinguished, renowned, may he be long spared to counsel and assist in the deliberations of the body!

The purposes of those who organized the Association were to protect and promote the interests of the American Medical profession, to maintain its honor and respectability, to advance its knowledge and to extend its usefulness.

That these desirable objects have been accomplished to a very great extent is a matter of history.

Through its moral influence it has united the great mass of physicians from Maine to Texas, and from the Atlantic to the Pacific in the bonds of fellowship, many of whom at great sacrifice of personal comfort and pecuniary interest, come to the annual meetings with hearty fraternal greeting for each other, and who, becoming touched as with a living coal of fire, renew their vows of faithfulness and loyalty to the cause in which they are engaged, and form the high and noble resolve to devote their time, talent and lives with still greater assiduity to their chosen profession.

If the Association had done nothing more than to have accomplished this unification of the Medical profession, it would have performed a service entitling it to an imperishable name. It has, however, been an active and powerful agent in the promotion of medical science and in the dissemination of useful knowledge. It has excited a spirit of improvement among the masses of the profession which nothing can stay.

Another great benefit conferred by the Association was the establishment of an *esprit de corps* in the profession, by the preparation and adoption of a Code of Ethics which comprises the great principles of truth, honor and justice, in regulating the relations of physicians to each other, to their patients and to the public. It should be and is the written law clearly defined and of acknowledged force and effect that prevails from one end of the country to the other. It forms an impassable barrier between the sheep and the goats, the clean and the unclean, the physician and the charlatan.

The strict observance of the Code has done more than anything else to maintain harmony in the profession and to elevate it in the public estimation. It embodies the true spirit of the Golden Rule,—"Do unto others as you would have others do unto you."

Every one who enters the profession should be provided with a copy of the Code and should make it the guide of his medical life. It will serve as a talisman to the young physician, and will be the best safe-guard against the snares and pitfalls which environ his pathway in his early professional life.

It would seem that every honorable and high minded member of the profession would be willing to endorse and be controlled in his intercourse with his medical brethren and the public by every article of the Code.

It is, however, to be regretted that there are some, who undoubtedly possess high order of talents, and are justly distinguished, who have an utter repugnance to the observance of certain parts of the Code and hold themselves aloof from the Association in consequence.

They, *probably* are as proud of our noble profession as we, and are equally as anxious for the advancement of its interests, but can they conscientiously affirm that the motives by which they are influenced are pure and unselfish? And should they, a small minority, put their opinion against the unbiased and unselfish judgment of the wisest and most experienced in the profession? And when, too, nine-tenths of that profession endorse

and are guided in their actions by the spirit and letter of the Code?

The chief object—indeed the fundamental idea of those who originated the Association was the improvement of the American system of medical education and the elevation of the standard of requirements for the professional degree. Never was there a greater expenditure of effort, illuminated with genius and learning, to accomplish these two great objects. Never was there more eloquent and philosophical reports to any organization than those presented by the committees appointed year after year, to this body; yet it seemed, after all efforts in that direction, as far from fruition as at its initial meeting. The resolutions proposed and adopted from time to time, were sufficiently pointed and admirably adapted to the end in view, but, unfortunately the Association had no legislative authority or power to enforce its enactments, and its moral suasion and influence was not great enough to move the college to accept their counsel.

In 1850 the medical colleges were invoked to meet and correct the great evils of the prevalent plan of teaching in this country, but they did not respond to the appeal. Since then, on various occasions, delegates from the medical colleges have met and discussed plans for the advancement of medical education but have failed, from want of coöperation and united action, to make the desired changes.

The idea of making such changes in the medical education of our youths in a country so diversified and of such extent, in a few years, was probably Utopian. Such radical and lasting changes can only be effected by the slow work of time. There has, however, been a gradual elevation in the standard of education, fully equal to the progress of the country in every other department of human learning.

To those of us who entered upon our medical studies twenty, thirty, or forty years ago it will be gratifying to visit any well organized medical college and witness the many improvements and the increased facilities for the instruction of its pupils. The extended curriculum of instruction, both in the didactic and clinical departments, the patient and painstaking work in the laboratories of Chemistry, Physiology, Pathology and experimental Therapeutics; the special instruction in the department of Surgery, Obstetrics and Gynecology and Ophthalmology; the resources afforded by hospitals and dispensaries, and the admirable arrangement for the prosecution of Practical Anatomy in the dissecting room, all attest the wonderful progress and advancement made in medical teaching since our pupilage.

I am ready to maintain that the advantages and facilities for medical instruction in our country even at the present time are quite equal to those of any other, and that our medical colleges have produced as able, learned and successful practitioners as ever graduated from other institutions, and while I am willing to admit that our transatlantic brothers have excelled us in experimental work, we excel in all practical departments of medicine. Chassaignac, the eminent Parisian surgeon, exclaimed a few years since, that America held the scepter of the Surgical world; and more recently Virchow, President of the late International Medical Congress, said, "The American Medical world to-day excels in Surgery, Midwifery and Dentistry."

What American physician is not overwhelmed with thankfulness when he remembers the pain and anguish which has been prevented through the great boon of anæsthesia, which in the providence of God was given to our country; who will ever tire of hearing of the great blessing conferred on suffering women by the genius and skill of McDowell, or of the marvellous changes made in Gynecological Surgery by the prolific brain and cunning hand of Sims.

Had America done nothing else than contribute these gifts to the world it would have been made famous for all time to come.

The great advance made in our medical education has been undoubtedly due to the frequent and very able discussions which have been held on the subject in this Association, and to the eloquent and philosophical reports of committees of education which have been so often appointed, and apparently with so little effect,—but the seed so wisely sown in the organization of this body have germinated and are now of hardy growth, and it is hoped in the near future will attain maturity and yield a harvest of abundant and perfect fruit.

The professional public have been aroused on the subject of a higher and better education than has been furnished by the old system, even, as improved by modern advancement. This professional sentiment has extended to medical teachers, and I believe that a majority of American medical colleges are now ready to acquiesce in the demands of the profession for a higher education, and that they will execute to the fullest extent the wishes of the Medical College Association as expressed at the meeting in Nashville last year. And when the medical colleges shall have entered upon the higher education which they have determined to do, the American Medical Association will have cause to congratulate itself in the accomplishment of the chief object of its organization.

The success of the Association in these and other purposes has been very great, even beyond the expectations of its most sanguine friends. "As a social and professional reunion of kindred spirits and great minds, its memories afford perennial

delight. It has given impetus to the progress of polity and science, it exercises moral suasion rather than authority, it has brought together a bright constellation of intellect, cemented the bonds of friendship among good men and true, and has formed a luminous track of light in the firmament of the Æsculapian heavens throughout the length and breadth of the land." It has passed safely through the perils of infancy, avoided the errors of youth, has entered upon the full estate of manhood, and now occupies an advanced position in the deliberative assemblies of the world. Constituted of delegates coming from every part of the country and representing every interest of the profession, it is really a great National Congress; it is the only legislative body of the profession which can regulate the action of its members and harmonize their conflicting interests; and it is full time that it should assume the power of legislating for the whole American profession and demand that its enactments be observed. That this power may be attained and exerted beneficially, greater efforts should be made to bring into the Association every prominent and influential member of the medical profession, especially those who have thought proper to alienate themselves from its deliberations. Let us ask them in the true spirit of conciliation to throw aside their narrow prejudices and renew their allegiance to the Association. Let us relegate all the asperities of the past to oblivion and remove the unpleasant feelings which have existed for the past four or five years, to the end that the medical profession of America may be, "Though distinct like the billows, yet one like the sea;" that it may be truthfully exclaimed, "Behold how good and how pleasant for brethren to dwell together in unity." Then, encouraged by the knowledge of the moral power of the Association, let us not be satisfied with what has been done in the past, but press on in the good work we have undertaken, toward perfection, securing as much beauty and finish for the body as is compatible with the imperfections of the human understanding.

Now that the College Association has adopted all the requirements for improved medical education, which the Association has been so long urging, and for which, in fact, it was established, it is eminently proper, and I would urgently press its importance on the members, to pass a resolution, that after the changes contemplated have gone into effect, no medical man who has received a degree from a college which has not adopted the improved method of teaching, and no professor nor attaché of such college shall be eligible as delegates or members of this Association.

This great moral support is due those colleges which so heartily took up the burden that the Association has carried for nearly half a century, and I hope it will be cheerfully accorded them.

It is a well known fact that a very large proportion of the members of this body at each session, come from the vicinity of the place of meeting, many of whom have never been present at the sessions before, and are ignorant of the parliamentary usages of the body, or entirely indifferent what business is before it, or how it is disposed of. So that a few only of its members shape and conduct the entire business. Indeed, the transaction of routine business of the Association is uninteresting and irksome. Sometimes in the discussion unpleasant feelings are engendered and animosities formed which interfere materially with the harmony necessary for the welfare and usefulness of the Association. I would therefore respectfully suggest, that all business matters of the Association, should be referred without discussion or comment, to an executive committee composed of two members to be appointed by every State Society in affiliation with this body, who, after mature deliberation, shall report them back to be adopted or rejected, as the Association may determine; and that the morning session, which has been heretofore consumed in the transaction of ordinary business, shall be occupied in the discussion of living, burning questions selected by the business or the nominating committee, and that members especially qualified to discuss the questions chosen, shall be selected a year in advance, that they may be thoroughly prepared for the duties assigned them. Such discussion will be greatly conducive to the advancement of medical science and will attract a great number of the best medical men who are not interested in the routine business of the Association, and who are consequently never present at the meetings.

To still further promote science and add to the interest of this body, I would suggest that prize essays provided for in the organization, and for so long a time ignored, shall again receive the attention they deserve. I feel sure that it is the sincere desire of every member of the Association that it progressively improves in the quality of the work it presents to the profession from year to year. There is, in my opinion, no exercise which will add more to the interest of the Association, or do more to promote science, than the presentation of essays which will be offered in competition for suitable prizes.

In this connection it may be well to call the attention of the Association to the fact, that original research and experimental investigation have not received the attention from American physicians, which their importance demands. Living as we do, in a comparatively new country, our energies have been directed to the promotion

of the practical, and more directly, useful departments of the profession than to the minute investigation of scientific subjects; and our government, while the most liberal and best under the sun, has never seemed to comprehend that the cause of science, especially medical science, would be greatly advanced, and its own honor proportionately increased, by the establishment of schools for original investigation and experimental research. It has not kept pace with the other enlightened governments in scientific enterprises. It is hardly to be expected, however, that in the rapidly changing political dynasties, the attention of our legislators could in the near future be directed to the advancement of pure science. This must, for the present, at least, be left to the progressive spirit which animates our universities, and to private laboratories which are being established in different sections of the country. Would it not be advisable for the Association to establish a Section of Experimental Research, to which young and enthusiastic devotees might be encouraged to make and repeat experiments in all the departments of medicine? It would certainly form a very interesting and useful Section, and would tend to advance science and add greatly to the interest of the Association.

It was a happy conception of one of our most distinguished presidents to make the establishment of an Association Journal the burden of his inaugural address, and so powerfully did he impress its importance upon the minds of the members of the Association, that a committee was at once appointed to take into consideration his suggestion, with the result that a weekly Journal was established to take the place of the annual volume of Transactions. That the change has been a valuable one none will deny. It has asserted and maintained the honor, dignity and power of the medical profession as a factor in civil life; it has tended to enlighten and strengthen the profession, and to lead it in the proper direction. Its design has been to represent in the broadest sense the true status and progress made in this country and to give expression to the thoughts, purposes, and will of American physicians. It has in the short period of its existence given evidence of its power in the advancement of its purposes. Yet it must be acknowledged, notwithstanding the great learning and untiring energy of its editors, together with the faithful coöperation of its trustees, that it has never attained the ideal excellence which should characterize the organ of this great body of physicians. It may require years to bring it to the desired standard, and it should be determined here to-day, that every effort shall be made to advance it to the highest standard. To effect so desirable an object it is necessary to make provision for an ample annual income. Nothing less than from seventy-five to one hundred thousand dollars should be considered ample. To many, this sum may seem chimerical, but it is not. If every member of this body would constitute himself a live, active agent to solicit subscriptions from his brother physicians living in his vicinity, and to assure himself that THE JOURNAL is on the table of every doctor within his reach; and if to this is added the income which may be derived from legitimate and properly selected advertisements, the necessary fund will be assured.

Next in importance to finance, is the selection of an editor, able, learned, highly educated, with ample editorial tact and business qualifications, who will devote all of his time and talent to his editorial duties. He should be empowered to spend money liberally in obtaining scientific material, original communications, translations and reviews from every part of the world. He should have absolute control in the selection of matter for THE JOURNAL; he should manage THE JOURNAL boldly, vigorously and with an eye single to the honor and glory of the profession.

To such an editor a salary should be given which would make him independent in a pecuniary point of view. Not less, I would suggest, than ten or fifteen thousand dollars should be paid to him annually. Then, with elegant paper, perfect typography and attractive binding, THE JOURNAL will add lustre to the Association and honor to the profession.

The necessary fund, which can be easily raised by proper exertion, will not only sustain THE JOURNAL in the best style, but will afford a sum in addition which can be used in many ways to the advantage of the Association.

The future location of THE JOURNAL is a matter of such importance as to require our careful consideration and mature deliberation. Its weal or woe may depend on the action of this meeting.

At an extraordinary session of the Board of Trustees of THE JOURNAL called to meet in Washington city last November, it was determined to submit the question of its removal from Chicago to Washington, to the action of the members of this session.

I would beg the delegates and members of the Association to consider well every side of this question before they commit themselves to a vote, and not act too hastily in the matter. THE JOURNAL has now had its home in Chicago for eight years. Its development and growth has been wonderfully rapid. It has already become the peer of any of the great weeklies of the country and if properly sustained by the profession and wisely and energetically conducted by its managers, it will become the recipient of the best thought of our own country and be the worthy exponent of the American profession. It is free from debt

and has funds sufficient to close the financial year and still leave a satisfactory balance in the treasury. It has been proven that it can be more economically published in Chicago than in Washington. The Board of Trustees several years since solicited and obtained estimates from two or more publishing houses in Philadelphia, Washington, New York and Chicago. These estimates were uniformly highest in Washington and lowest in Chicago.

Chicago is a geographical and railroad centre not excelled in facilities for rapid mail distribution. It is also a great medical and surgical centre and can place at the disposal of THE JOURNAL a wealth of resource, second to no other city in the Union. It is the best place for advertisers to reach the profession in the Mississippi Valley and the great North-west, and if THE JOURNAL should be removed to Washington, it will lose many very lucrative advertisements and will be brought into direct and sharp competition with six important weekly journals, several sustained by large and influential book houses, which occupy the field between Washington and Boston.

Washington is in no sense an important scientific, educational or professional centre; but it *is* the great centre of American politics, to which everything is made subordinate; and it would be impossible if THE JOURNAL should be published here, to prevent its becoming contaminated by the political air, with which it would be surrounded. Washington has never been a healthy locality for medical journals, for every journal which has been published within its boundaries, has died early from inanition. I do not think that it is a better locality than Chicago that is wanting, but a better Journal than we have.

Let us then as members of a profession engaged in the most beneficent and humane calling known to man, in the language of the immortal Jackson, swear "by the ETERNAL" that THE JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION, wherever it may be located, shall be made and kept equal in excellence with any journal published in the world.

It is now time to enter upon the duties which have called us together. Let us endeavor to discharge them in a spirit of conciliation and justice. Let us act with such prudence and judgment in this meeting as will tend to unite more closely the members of this great National family. Let us not forget how rich an heritage has been bequeathed to us by our predecessors which we should make more valuable and transmit to posterity. Let us indulge the hope that our labors during the present session will not only sustain the advance already made in medical science, but will carry us still further onward in the great road of progress.

THE ADDRESS ON SURGERY.

Delivered at the Forty-second Annual Meeting of the American Medical Association, held at Washington, D.C., May, 1891.

BY JOSEPH M. MATHEWS,
OF LOUISVILLE, KY.

I am familiar with the fact that it is expected that the general address on surgery read to this Association shall be a *résumé* of the advances made in each and every department of the science and art of surgery, since the previous meeting. To have gone over this entire field would have consumed more of your valuable time than you would like to grant, and involved a task too burdensome for me to undertake. I, therefore, determined to select some single subject for my address. I was then confronted by the fact that every single subject of importance would be fully discussed in the section on surgery; hence I was embarassed in selecting one, for the reason that I wished to edify rather than fatigue you. This, then, was the rub. Having confined myself for fifteen years to a special line of practice, it occurred to me that it would be best to select some subject relating thereto, and give my individual opinion and experience in regard to it. Then, too, I had the consolation of believing that no paper of a similar topic would be read before the Association. I have, therefore, selected as my subject,

STRICTURE OF THE RECTUM—ITS ETIOLOGY, PATHOLOGY, SYMPTOMOLOGY, DIAGNOSIS AND TREATMENT.

I realize, in discussing this subject, I shall take positions contrary to the accepted teachings of the day, but I shall not beg for pardon for so doing. I take it that the one great object of the meetings of this Association is to elucidate and discuss subjects that are in doubt, those that are mooted, not admitted. The caption of my address may sound homely, but the importance of it as a surgical subject should commend the respect and attention of every surgeon here. Its frequency, its terrors, and its dangers, appeal to us for relief.

Etiology.—The following classification of the varieties of stricture of the rectum is given by Dr. Kelsey. I believe that for my purpose it is the best classification, as it embraces the varieties as given and agreed to by many different authors.

Congenital:
1. Complete,
2. Partial.

Acquired:
1. Spasm. *a.* Dysenteric.
2. Pressure from without. *b.* Tubercular.
3. Non-venereal. *c.* Inflammatory. *d.* Traumatic.
4. Venereal. *a.* Ulceration (either chancroidal, secondary or tertiary.)
5. Cancer. *b.* Due to unnatural vice. *c.* Neoplastic (gummata, anorectal syphiloma.)

The first great division, it will be noticed, is congenital and acquired stricture. In writing of, or dealing with stricture, the idea intended to be conveyed is of a pathological change in tissues,

etc., a deviation from the natural brought about by disease; hence I object to the consideration of congenital malformations of the rectum, or to define them under the head of strictures of the same, for the reason that it is misleading to do so. It will be more to the point to call these, atresias of the gut. Therefore, I shall dismiss this part of the division that he has made. Indeed, exception could also be made to the second division of this grand classification, namely, acquired. I am aware of the fact that the term is often used in the sense herein applied, but to my mind a better term or classification could be used. It is very easy to understand how one can acquire a stricture, the result of venery, but it will be difficult to understand how one could acquire a spasmodic or cancerous stricture. But I will adopt, for the sake of discussion, the above classification, leaving out the congenital variety.

1. *Spasm.*—To this form of stricture I shall prefer two objections. First, if it be true that such condition ever exists, which I doubt, then it should not be classed as stricture at all, for the reason that no pathological change is manifest to constitute a stricture, and no treatment could be given it *per se*. In other words, it would be a symptom of some lesion or trouble outside of the one called stricture. Second, I believe that from the anatomical construction of the rectum, it would be utterly impossible for its lumen to be so constricted as to be perceptible as an obstruction, by spasmodic contraction of its muscular fibres. I might add as a third reason, that in all my examinations of this part of the gut, I have never seen a spasmodic contraction that could be called a stricture.

2. *Dysenteric.*—Although it is frequently stated that dysentery is a common cause of stricture of the rectum, I have never seen cases sufficient to convince me of the truth of the statement, indeed that it was a cause at all. I have many times seen patients who gave me a history of having had dysentery and were treated for a long time for the affection, but a close scrutiny of the case revealed the fact that the so called dysentery was caused by an already existing stricture and ulceration, the rule here being reversed,—that dysentery was the result, not the cause. If dysentery really be a cause of stricture of the rectum, how very often we would expect to meet with it in our practice, considering the great number of people who have dysentery, especially in the warmer climates. Again, practitioners of medicine know that ulceration proper very seldom exists in the rectum during or after attacks of dysentery. The sloughing in these cases occurs from the gut above the rectum. I do not deny, but I am not convinced, that ulceration may sometimes be caused by repeated dysenteries or diarrhœa, but my experience has not taught me that they are frequent, by any means. If a long continued irritation is kept up in the rectum, from any cause, the result would be, of course, an inflammatory exudate, resulting perhaps in ulceration and stricture; but I must confess that in searching for this as a cause, the road to a conclusion has not been plain enough for me to put dysentery in the list as a cause at all for stricture of the rectum. If this disease is a common cause of stricture, as asserted by so many, it occurs to me that the trouble would be often found in the veterans of war. Indeed, I could not imagine a more ideal case for a pension than the existence of stricture of the rectum, the result of a dysentery contracted while in the service. Yet the pension records are singularly silent on this point. At a late meeting of the Louisville Clinical Society, Prof. John A. Ouchterlony, a distinguished pathologist and teacher, in discussing the subject of stricture of the rectum, said:

> I call to mind a dead house experience extending over many years. During the war I made post mortem examinations upon hundreds of cases who died of dysentery, the most malignant forms of the disease, as all will attest whose observations extend back to war times, and I cannot remember to have ever seen a stricture of the rectum as a result of dysentery. In the two hospitals, to which I was pathologist, there were eleven hundred and fifty beds, and we sometimes made as many as five or six post-mortems a day. After the close of the war, I was for many years pathologist to the City Hospital, but in all my dead house experience, I never saw a stricture of the rectum caused by dysentery.

These are the remarks of a very close observer, and my experience certainly coincides with his.

3. *Tubercular.*—Since the discovery of the tubercle bacilli, and the demonstrations that convince us of the effect on the tissues, etc., it is self evident that tuberculosis is often met with in the mucous membrane and the structures of the rectum. If stricture *and* ulceration is the term used, I could make no objection to the classification of tuberculosis as a cause of ulceration. That ulceration frequently results from this diathesis or dyscrasia no one could doubt, but that the coincident stricture follows, as from other well known causes, notably syphilis, I cannot agree. The disposition of tuberculous tissue everywhere is to break down. Before the capacious rectum is filled with tubercular deposit sufficient to stricture it, it will have broken down from ulceration, etc., and it must be by deposition only that we can conceive of stricture from this cause; because cicatrization is so seldom, and so feeble, in these parts that it would be the rarest accident to find it. In no instance have I ever seen a stricture of the bronchi as the result of tuberculosis. There would be just as much reason to expect it here, or indeed more so, than in the rectum.

4. *Inflammatory.*—This term is so broad and comprehensive that we must perforce of reason admit it as a cause of stricture of the gut, indeed as the one grand and common cause; for if stric-

tures exist from whatever cause, be it trauma, pressure, venery, dysentery, cancer, tubercle, syphilis, ulceration, or what not, it is inevitably due to the processes and products of inflammation—in no other way can a stricture be formed. It might be argued that a lesion or wound existing in the bowel by the reparative process heals and leaves cicatricial tissue, and that stricture was the result of the cicatrix, and not to plastic infiltration of the tissue. In answer, I would say that there could have been no cicatrization if there had been no inflammatory process. Hence, inflammation being the cause of the cicatrix, was in truth the cause of the stricture. It is said, "Any severe form of proctitis resulting in ulceration may be a cause of stricture." To this I freely assent, but the most difficult part of the whole matter is to tell the cause of the proctitis, which is inflammation. It is not to the proposition that I object, but to the supposed cause. For instance, in naming several, the following is given as a cause of stricture: "Erosion and ulceration of hæmorrhoidal tumors." Now, in the nature of things, how can this be true? A hæmorrhoid is, in fact, a tumor, and by friction the mucous membrane on the tumor can become ulcerated. Suppose it does, how can that ulceration produce a stricture of the rectum? As we have intimated, strictures may result from two pathological conditions. First, from a deposition of plasma, causing an obstruction, or second, by cicatrization, causing a stricture. Can either of these conditions result from hæmorrhoidal tumors being ulcerated? Certainly not. The inflammatory deposit would only involve the tumor, and a cicatrix on top of a pile would not amount to a stricture.

5. *Traumatism.*—Under this head the authors include ulceration following operations or wounds of the rectum, and cite the surgical operation done for hæmorrhoids and fistula in ano. In all my practice, I have never seen such result follow either operation. I can understand how the cicatrix resulting from the removal of too much skin from the anal region might cause a stricture of the anus. My friend Dr. W. O. Roberts, of Louisville, has told me recently, of operating upon a patient of this kind, the original operation for hæmorrhoids having been done by an inexperienced hand. I cannot understand how a surgeon used to operating in this region, would do an operation that would result in a stricture. But traumatic strictures are, in fact, inflammatory strictures. Inflammation is the result of trauma, so one class would include both. For brevity, this would be best.

6. *Venereal.*—"Without admitting too much," says one author, "it may be safely said that, beyond dispute, there are three forms of well recognized venereal diseases in the rectum, which may result in stricture. These are chancroidal, secondary and tertiary ulcerations, either simple, traumatic, or the result of direct inoculation, and an unusual form of tertiary disease of the general nature of gummatous deposit variously described by different authors, and by Fournier as ano-rectal syphiloma." This author leads us to infer that these three venereal causes, namely chancroidal, secondary and tertiary ulcerations, are the most infrequent way that stricture of the rectum can be produced by venery. Allingham reports that out of seventy patients suffering with stricture of the rectum, thirty-five of them had a history of syphilis. I have frequently said that I believe that more than one-half of the strictures met with in the rectum were the result of syphilis. I have often asserted that in no single instance have I ever seen a stricture of the rectum caused by the healing of a soft sore. I do not believe that it can occur. In this opinion I am partially sustained by Allingham, James R. Lane, Alfred Cooper, Coulson, Christopher Heath and others. These three causes are alleged by many to produce their effect by simple trauma or direct inoculation. Neither of these can be true. If so, certainly not one in ten thousand cases. Granting that the soft sore could produce an ulceration that might end in stricture, how, I would ask, can the aforesaid pus get into the rectum? It may have occurred, it is not impossible, but granting that it did by direct contact, I do not believe that it would result in a stricture. Instead of secondary syphilis being an unusual cause of stricture of the rectum, I assert that it is the usual and only form that we find this disease producing or causing stricture of the rectum. Ricord, Fournier, Heath and others believe this, and Mr. Bryant, in his excellent practice on surgery, ascribes these ulcerations and strictures of the rectum as "mainly syphilitic," and says: "Foreign authors describe chancroidal diseases of the rectum, venereal but not syphilitic. In this country it is hardly recognized." I certainly agree with this author. To conclude, if I were asked, what is the prime cause of stricture of the rectum, I would answer *inflammation*. But what causes the inflammation? In many cases I do not know, but ordinarily syphilis, cancer and trauma—if by trauma can be meant a wound or lesion from any or many causes. Outside of the two first named, cancer and syphilis, I am satisfied that no one can tell the cause that originates the stricture. I wish to reiterate, that outside of these two well recognized causes for stricture of the rectum, I am not prepared to admit any other as a well known, recognized, indisputable cause.

Pathology.—In delivering the Bradshawe lecture before the Royal College of Surgeons, London, Mr. Thomas Bryant selected as his subject, "Colotomy." He said:

> But as a means for giving relief to patients from chronic intestinal organic ulcerations or obstruction from whatever cause, colotomy was generally, and indeed I may

say is still, too much regarded as a *dernier ressort*, and as a consequence, it was, as a rule, only carried out when all other measures had been tried and proved to be useless. This position I, in common with some few other surgeons, have, however, never accepted. We have regarded it as the best means the surgeon has at his disposition for the relief of rectal obstruction from cancers and every disease which is not otherwise removable, and experience has proved that life may by it be saved, when the disease is not cancerous, and prolonged even for years when it is so.

Turning to page 605 in Wyeth's Text Book on Surgery, we read: "In stricture of the rectum, when all other measures fail, colotomy is the last resort." Here are two diverse views, by two very distinguished authors. Which is correct? I am decidedly inclined to Dr. Wyeth's opinion (if colotomy is performed at all), and although he has been content with the bare statement, without argument, I shall in a few words give you my reasons for differing from Mr. Bryant, in his statement and proposition. I quite agree with him in the preference given to lumbar over inguinal colotomy, but I beg to differ as to the need for the operation, and base my belief on the pathology of the disease. Instead of admitting his premise that colotomy is called for the relief of rectal inflammation by cancer and other diseases, and should be performed early in the disease, I shall contend that such a procedure is warranted only in the rarest cases, and then as a *dernier ressort* only, which he denies. My conclusions are based upon an observation of several hundred cases of so-called obstruction of the rectum. I shall not found my objections upon the dangers that attend the operation, although every surgeon will admit that some danger attends it. I recognize the fact that, under antiseptic surgery, the mortality attending these, as well as all other surgical operations, is reduced. However, this admission plays no part in rebutting other arguments that are urged for the operation. I will be permitted to remark that, in my opinion, it has become too much the custom, or fad, to do this operation in cases where there is no possible chance of doing the patient any good. Indeed it has become so common, that the moment cancer of the rectum is diagnosed, colotomy is resorted to. Mr. Bryant states two distinct propositions, namely, "First, the immediate success or failure of the operation turns but little upon the operation itself, if well performed, but upon two main points, the first being the local condition of the bowel above the seat of obstruction, and the second, upon the general condition and age of the patient."

Some surgeon once said, that the reason laparotomies for gunshot wounds showed such a low per cent. of recoveries, was that too many were attempting the operation. Mr. Bryant can very well say that the immediate success or failure of colotomy turns but little upon the operation, if it is well performed. The trouble is that if his premise be true, that this operation should not be considered as a *dernier ressort* in stricture of the rectum, but that it should be resorted to early in the disease, and is the best of all procedures, too many men, accepting his dictum as true, will be doing the operation, when less dangerous methods might accomplish the same results. As to his two main points to be considered before doing the operation, first the local condition of the bowel above the seat of obstruction, I take it that he means whether or not the bowel above the seat of obstruction is invaded by the disease, or if, in consequence of the disease below, has suffered. In my opinion it would have been more to the point to have said, the local condition of the bowel both *above* and below the seat of obstruction. He says: "If from procrastination, serious intestinal changes have taken place before relief is present, recovery is hardly to be expected." Now I suppose that the distinguished author refers here to cancer or syphilis as the serious causes of intestinal changes. It is quite agreed that these two diseases account for fully two thirds of the obstructions or strictures of the rectum, which, as Mr. Bryant and others would say, would call for colotomy. Suppose the gut above the stricture was not invaded or changed at all, but that below the stricture, incipient it may be, there is a slight infiltration by cancerous deposit, is the operation justifiable? I certainly cannot agree that it is. It is admitted that a colotomy is a loathing and disgusting thing. Patients with cancer of the rectum live from three to six years. Many in my practice have lived five years after the disease was first observed. Why subject these people to such an operation during the incipiency of the disease, when it does not stop it? Again, are we quite certain that there is an infallible sign of cancer? In quite a number of cases I have taken specimens from rectal growths, had them examined by microscopists, and pronounced cancer, whose subsequent history revealed the fact that it was not cancer at all. Then, too, in the early stages of cancer, there is not sufficient clinical evidence to base an opinion upon. Certainly for a benign and incipient stricture, or obstruction, if you please, in this locality, colotomy would not be advised.

Again, if the disease be cancerous, in its incipiency, or otherwise, can the operation of colotomy cure it? It might be, as Mr. Bryant suggests, the operation could be done much more successfully while the general health is in good condition, but it is not whether one can perform colotomy successfully or not, the prime question is, what good will it do? A surgeon may do a beautiful operation for stone in the bladder and get the stone, but the patient dies. How then, I would ask, can the establishing of an artificial anus in the side, in any way cure or arrest the in-

roads of cancer in the rectum. If, then, it is granted that the disease being constitutional will go on regardless of the opening in the side, I would ask can the colotomy prolong life of the cancerous patient? In no way possible can it do so, but one, that is by preventing one source of irritation, namely, the passage of fæces over the cancerous mass. The argument would be, the more irritation, the more deposit.

In my opinion, this is of very little importance, or consideration in cancer of the rectum. Malignant growths increase by an inherent power, a deposition, infiltration, etc., intrinsic, not extrinsic. They will exist in the rectum a long time, acquiring a great size, involving, perhaps, the whole circumference of the gut before the mucous membrane is ulcerated, notwithstanding this "great irritation" is constantly kept up. I dare say if a cancer of the same kind and proportion, in the same character of subject, could be watched each day, in one colotomy had been performed, the other without, that no difference could be observed in the rate of progress that was being made. I am forced to this belief after an observation of many years, covering many cases. As I have before stated, my patients have generally lived from four to six years with cancer of the rectum, without colotomy. Do they live any longer with colotomy? In other words, can any one say, because, colotomy being performed, a patient lives four or six years, that the colotomy was the cause of prolonging his life?

Again, it is claimed that by colotomy much of the pain is the rectum is relieved, in that the fæces having been directed from their natural course. In some instances this may be true, but the rule will not hold good in all. I have known patients to suffer equally as much with pain after, as before the operation, nor is it always true that the fæces are diverted from the natural channel. Much discomfort is often complained of by the patient, after a colotomy, from the fæces lodging in the mass, or strictured surface.

In a paper read by Mr. Jessop, on the treatment of cancer of the rectum, at the Leeds meeting of the British Medical Association, he said: "In cancer of the rectum, the constriction in the majority of cases can be got over for a time, by injections, the introduction of the finger, or bougies, the use of laxatives and the like." This has certainly been my observation. Indeed I have seen many cases where the patient never complained of even constipation or obstruction. Add to this that many patients of the kind complain of but little, if any pain, especially if the growth is situated above the sphincter muscle, it lessens the cases materially which would call for colotomy.

I cannot agree with Mr. Bryant in his statement that the operation is demanded for the purpose of relieving the local distress, admitting, as he does, that when the disease is in the lower part of the rectum that obstruction seldoms occurs. I have seven cases of cancer of the rectum now under observation, and in but one is pain a factor at all. Admitting that pain is a prominent symptom, colotomy does not bring that radical relief which would justify its being done. We have in opium a remedy which will quiet pain effectually, and if the argument be used that we would make an habitué of the patient, I would remark, what is the difference if he is to die so soon? As I have observed, pain in cancer is inherent, caused by the local affection of the nerves, and is not controlled by extraneous circumstances. Hence, of what account is opening the gut at a distant point, if pain is not a great factor in the disease? If, in answer, it is said that it prevents the irritation and pain caused by the passage of the fæces, I would answer that in many cases this does not increase the pain, and if it does, dilatation will materially prevent it. I saw, this day, in consultation, a lady whose lower rectum, including buttocks, labia, etc., were involved, the gut for six inches tightly strictured, and when asked how much pain she suffered, answered, "Oh, very little," and said that the fæcal discharges caused her no trouble.

If, as some would have us believe, colotomy would prevent the extension of the disease, and its consequences, such as involvement of the bladder, vagina, etc., I would ask how is colotomy to prevent it? It is not the passage of fæcal matter over the affected parts that causes this result, but rather the nature of the disease to infiltrate, and break down the tissue. If a cancerous growth is situated above the sphincter muscle, its tendency is to extend upwards, and in this event pain is not great, unless some other organs are affected. It is not uncommon that patients come to my office to consult me for some trivial rectal affection and I find, upon examination, a cancerous mass, extending all around the rectum, pain being scarcely a symptom in these cases. Of what value would colotomy be here? Hence I am forced to the conclusions that the operation is not warrantable simply because cancer is found in the rectum, either in an incipient or confirmed state; nor for the relief of pain simply, unless other complications exist, for we have medicines that will relieve pain; nor to prevent invasions by the disease, because it would fail of its purpose. Infiltration and pathological change cannot be overcome by colotomy. Nor for obstruction or fear of obstruction in the lower rectum, because, as Mr. Jessop says, this obstruction seldom takes place, and if it does, it can be relieved by dilatation and other methods. Lastly, I do not believe that colotomy should ever be done for the obstruction of the rectum by cancer, save, perhaps, in a few exceptional cases, and then only as a *dernier ressort*. Yet in every instance before

the operation is done, the nature of it and its consequence should be fully explained to the patient, with the assurance that it cannot possibly effect a cure. It is a well recognized fact that colotomy is advised and practiced by many, for other ulcerations and constrictions of the rectum, beside those of a cancerous nature. Except under certain conditions, I shall object to this procedure with just as much emphasis as to the operation in malignant stricture.

I have stated, in this article, that I do not hold to the theory of chancroidal ulceration, as so vehemently advocated by Zeigler, Mason, Kelsey, Gosselin and others. My reasons have already been given, but I do hold that syphilis is responsible for more than one-half the cases of stricture of the rectum, and in the manner I have described. But to do a colotomy simply because a patient has a syphilitic stricture of the rectum, is, I believe, unjustifiable. As a proposition, then, I shall maintain that where a syphilitic stricture or strictures exist in the rectum, located within four inches of the external sphincter muscle, colotomy should not be done. In supporting this statement, I desire to say that in my opinion, a stricture located within the distance named, can be treated more successfully by other methods.

Before leaving the subject of colotomy, as one of, if not the most important of all methods of treatment for stricture of the rectum, from whatever cause, I beg to call to mind that the statistics of Erckelen carefully compiled in 1884, and shown by Treves that 38 in every 100 cases of lumbar, and 46 in every 100 cases of iliac colotomy died within twenty-one days after the operation. This statement speaks for itself. Stricture from benign causes located within the distance that I have named, should be dealt with in a similar way.

Symptoms.—The early symptoms of stricture in the rectum are very obscure and confusing. Indeed no stricture exists at all in the pathological changes going on in the gut which conduce to this state. The great trouble is that the early symptoms are so masked or entirely *nil*, that no attention is paid to them by the patient, that when he is forced to consult a physician a very decided stricture may exist. The changes made manifest in the rectum are those of inflammation and, if from cancer, the condition of the blood vessels, and the gradual deposit of the morbid material, together with infiltration of the tissue, goes on so slowly and insiduously, that for a long time there are really no symptoms. I have seen many cases where the first symptom noticed was a so-called constipation, obstipation would be a better word, and upon the introduction of the finger a tight constriction could be felt. This may apply to any form of stricture. I have under observation now three cases of this kind. The first symptoms of stricture then are not the discharge of bloody pus, etc., indicative of ulceration, that some describe. Therefore, I must differ from those who place the symptoms of ulceration first, and those of constriction afterward. Ulceration cannot take place, together with the symptoms incident thereto, a discharge of blood, pus or muco-purulent, until the changes of inflammation have been such that the mucous membrane and sub-mucous tissues have undergone that change which constitutes ulceration. When this latter condition is established, we have the characteristic discharges—diarrhœa, flatus, the muco-purulent discharge, or rather a muco-bloody discharge first, succeeded eventually by a purulent discharge, and alternating diarrhœa and constipation. The bearing down sensation, together with tenesmus, a reflected pain to the back and down the thighs, an irritation of the kidneys and bladder, and uncomfortable feeling always about the rectum. A passage of small bits, or tape-like actions, are all indications of the disease. I am persuaded that oftentimes stricture is diagnosed from this characteristic tape-like action, when in reality the moulding is done by the sphincter muscle in an irritable state, and that no stricture in reality exists. I am satisfied, too, that many cases of so called chronic constipation are due to a narrowing of the lumen of the gut from this cause. This has occurred so often in my practice that I am now in the habit of examining the rectum in every case of chronic constipation. This same rule holds good in cases of supposed dysentery, for, as I have observed, dysentery is but one of the symptoms of stricture, and caused by it.

I have had but two cases of acute obstruction caused by the chronic condition of the stricture. One in the case of a young lady who failed to report to me as often as necessary for a dilatation of the stricture (she would not consent to an operation) and during a summer outing took sick and died from an acute obstruction. The other was a young married woman, in the practice of one of our local physicians. An operation with the knife relieved her. Acute obstruction, as the first symptom of stricture, I have never seen, although I have examined a number of patients who complained of constipation only, who upon being examined, showed a decided stricture that the smallest finger could not pass. In one instance I found a close stricture at the entrance of the sigmoid flexure. It is truly wonderful to see patients who have strictures of the smallest calibre, who seem to enjoy perfect health, and whose physical proportions and development are not hurt in the least. It must not be forgotten, however, that these are dangerous conditions and constantly imperil the life of the patient.

Diagnosis.—When the stricture is within four inches of the sphincter muscle, it is easily diagnosed, be it malignant, benign or syphilitic—the

finger will detect it. It is a very different matter, however, to determine its character, and yet, to a certain extent, the treatment depends upon it. I desire to quote from Kelsey the following statement:

> There is an old and deeply rooted idea in the minds of the profession, that a stricture of the rectum must be either cancerous or syphilitic—an idea founded on error and capable of doing much harm and injustice to innocent people. Again and again I have been able to give great comfort to women suffering from this disease by denying the correctness of this idea, and in my own practice, the fact that a stricture is not cancerous adds little weight to the idea that it may be syphilitic.

This is so diametrically opposed to my views and observations that I desire to say that, in my opinion, fully sixty per cent. of the strictures of the rectum are due to syphilis. Not venereal in the sense that many would have us believe, namely: by the infection of the rectum by canchrous pus, or by direct contact, but as a secondary deposit, the result of constitutional disease. There are but few authors to-day that deny this fact, but in admitting it, class these as exceptional cases. By a late estimate it is calculated that five million people in the United States are subjects of constitutional syphilis. If it is admitted that one single case of stricture of the rectum can result from this constitutional disease, it admits the argument. Then taking into consideration the great number affected with it, is it any wonder that we should have the per cent. named as suffering from this manifestation in the rectum? Mr. Allingham, in tabulating his cases of stricture, says: "Thus out of the total number of 99 patients, 52 or more were syphilitic."

As a means of diagnosis, the clinical history and observation of the case has much to do with forming a correct opinion. If it is ascertained that the patient has constitutional syphilis, I would consider that it was a strong point gained. I do not wish to be understood as saying that in every case where both syphilis and stricture exist, that the latter was caused by the former, but undoubtedly in the vast majority of cases this is true. Indeed, so firm am I in this belief, that if it is a question between cancer or no cancer, and it is decided that it is not malignant, 99 out of every 100 cases will prove to be syphilitic; for the reason that stricture, the result of benign ulceration, does not resemble in the least stricture from malignant deposition. To the contrary, syphilitic stricture does, to a degree, in its pathology, resemble malignant growths. To be plainer, malignant disease, and syphilitic disease, invade the rectum as a deposit, infiltration of the sub-mucous tissues, etc. Ulceration here is secondary to the deposit caused by the friction of the passage of fæces, or the breaking down of the tissue, the result of the disease *per se*. Benign ulceration begins with the damage done to the mucous membrane, and the plastic infiltration is secondary to it, the reverse of both the malignant and specific disease.

Again, as a diagnostic symptom, the touch reveals a great deal. Allingham says: "There is something peculiar about the feeling of cancer, which the operator's finger rarely mistakes even for simple indurated ulceration." This is the fact, yet if I was called on to describe it, I could not. It is said by many authors that the peculiar smell or odor of cancer is pathognomonic. I am certain that I have seen many cancers that did not evidence this peculiar odor. Great stress is also laid on the disposition of malignant growths to bleed, especially when touched or handled. I am just as sure that I have seen many cases of cancer that had no such disposition. The swollen or enlarged glands in the inguinal region cannot be taken as a postive sign or indication of cancer in the rectum, from the fact that they are swollen in many cases of benign ulceration and inflammation. I will again state that pain, in my opinion, is made too great a symptom of cancer. It depends altogether upon circumstances whether it exists to any degree or not. In cancer the nodular form of the mass is more apparent than in any other trouble. In syphilis, the induration is more even and extends with more regularity, and after a time is of a fibrous character and is so indicated to the touch. In simple ulceration, the stricture is apt to be annular.

As a method of diagnosis, I object to the use of rectal bougies, either metal, soft, or hard rubber, to which so many authors call attention. They are exceedingly dangerous, and accomplish no earthly good. I have known two patients killed by the attempt to introduce the common hard rubber English bougie within a stricture of the rectum. The common seat of stricture is within reach of the finger. It is the rarest thing that one is ever found in the movable gut. Granting that one exists there, if there is not a total obstruction, what is the use of an exploration with a dangerous instrument, when the finding of the stricture, or the supposed finding of it would result in no good. A stricture located in the movable gut cannot be dilated. If it be total occlusion or obstruction, it calls for very different treatment from this.

Treatment.—In considering the treatment of this very formidable disease, I shall adhere in the strictest sense to the pathological condition, namely, a stricture. This entirely rules out the treatment of proctitis, or the subsequent ulceration, which is one cause of stricture, and brings us directly to the means of treating that which is the result of said causes. It must be granted that many times ulcerations, which would eventuate in stricture, are cured before that condition results. This cannot hold good in cancer. Can it in syphilis? I doubt it. In the great majority of cases, we are confronted at the onset with stric-

ture, not with the ulceration, so insidious is the disease. The methods practiced to-day for treating stricture of the rectum are:

1. Dilatation.
2. Incision
3. Electrolysis, and raclage.
4. Excision.
5. Colotomy.

Of course, under the division I have made, we rule out general treatment.

Dilatation.—Kelsey in speaking of dilatation, says.

"By dilatation I mean gradual stretching, not forcible divulsion," and adds that the latter is seldom applicable. I must dissent to these views. The gradual dilatation of stricture is objectionable, for the reason that by this form of repeated irritation, more plasma is thrown out and the strictured surface increases. It may be true that some temporary relief is afforded, but upon the contraction of the tissue, which is sure to take place, we have lost more than we have gained. I do not hold to the view that by the passing of bougies through the strictured surface absorption of the tissue is caused, I believe that the converse is true. Why forcible divulsion is seldom appli cable in these cases, I cannot understand. If a fibrous stricture exists, I am sure that forcible divulsion is the best method. In other words, we do, in a few minutes, by immediate dilatation, what it would take weeks to accomplish by gradual dilatation. The fear, in the past, has been hæmorrhage in this operation. To-day we do not fear it, because we understand how to control it. Therefore, I would put but little stress, or no stress at all, upon treating stricture, of whatever kind, by bougies. In fibrous stricture it would do no good; in the malignant one, it would be dangerous. I have abandoned their use altogether.

In 1878 I read a paper before the Kentucky State Medical Society, in which I reported a case of close stricture at the entrance of the sigmoid flexure. The plan adopted was to break the sphincter muscle, introduce the hand and arm into the rectum, and reaching the stricture, which barely admitted my index finger, I made a cone of my four fingers and forcibly pushed them through the stricture. It gave way before them, and although great shock supervened, the recovery from immediate danger was effectual. I am satisfied that a more perfect and radical relief was obtained than if I had done colotomy. Gradual dilatation here was out of the question. I am very positive then in saying that if dilatation of a stricture of the rectum is decided upon, let it be a forcible and radical one.

Incision.—I am very partial to incision, or incisions, for the relief of stricture of the rectum. Of the two operations recommended, internal and external posterior linear proctotomy, I much prefer the internal. It is urged for the external, which consists of not only going through the strictured surface, but also in dividing the sphincter muscle, that it is all important to get the necessary drainage. I do not think so, and if I did, I believe the ill-effects of dividing the sphincters outweigh the matter of drainage. I cannot believe that the internal incision is as dangerous as it is represented to be by some authors. My plan is to introduce a three or four valve speculum, and, after dilating sufficiently for the purpose, a long sharp knife is used to divide the constrictions of fibrous tissue, down to a healthy base—not only in the median line, but in several places around the circumference of the gut. I then place a tampon, through which I have inserted a metallic tube for drainage, and the escape of gases. This tampon is aseptic, and usually dusted with powdered persulphate of iron. On the fourth day it is removed and the rectum irrigated with a mercuric solution. If the operation is done effectually, I have never seen the necessity of employing the bougies afterward, for the purpose of dilatation. Patients are averse to their use, and they do not accomplish the good claimed for them. My objection to the external operation, although I have practiced it often, is that to divide the sphincters when all the tissues are in a diseased condition invites non-union, and incontinence is nearly certain to follow. The suggestion of Weir, to confine the incision to the stricture and then to drain the incision by a tube brought out through the skin, at the tip of the coccyx, I do not think will accomplish the purpose in many cases; besides, it leaves a channel which may not heal. To divide the sphincters and then employ three or four deep provisional wire sutures between the anus and the strictures, leaving them loose and stuffing the incision with charpie, after the manner of Kelsey, I think unwise and unsurgical. It is said that one great danger of the operation is septic peri-proctitis, but under antiseptic precautions, the danger, in my opinion, is reduced to a minimum. In one case of malignant disease, in which I did the external operation, rapid sepsis took place, and the patient died in twenty-four hours. I do not think either the internal or external operation should be done for malignant growths, unless total, or nearly total, occlusion has taken place. In all cases of non-malignant stricture, syphilitic or simple, either the internal linear proctotomy of the French surgeons, or the external operation as practiced by many, is far more preferable to excision, or colotomy.

Electrolysis.—It does appear that where we can so effectually go through a stricture by linear proctotomy at one sitting, that it would be useless to attempt so slow a process as electrolysis. After a careful review of the subject, I cannot believe that any benefit obtained is brought about by the dilatation from the electrodes used, as

suggested by some. If there be a benefit, in fact, it must be attributed rather to what is claimed for it—partial destruction of tissue by cauterization. To claim radical cures by this method, I must admit, seems untheoretical, if not unsurgical, and yet Dr. Newman and others report many cases of stricture cured by this method. In a paper read before this Association and published in THE JOURNAL, he makes the following conclusion:

1. Electrolysis in the treatment of stricture of the rectum is not a panacea; on the contrary failures may happen, and probably will, if the stricture is due to carcinoma. 2. Electrolysis will give improvement to the stricture when all other methods have failed. 3. Electrolysis will cure a certain percentage of cases, without relapse, and without the necessity of an after treatment or using bougies. 4. The best agents for a cure are through the fibrous inflammatory stricture.

Having no personal experience with this method of treating a stricture of the rectum, I am not prepared either to advocate it or disprove the statements made.

Excision.—I think a better term to employ here would be extirpation. Excision of a stricture of the rectum conveys but a little idea of the operation. I cannot appreciate the idea of excising a benign stricture, not from any serious doubts as to whether it could be done or not, or any dangers attending the operation, but there are methods so much simpler in their nature for the relief of benign strictures, that I cannot conceive of a surgeon attempting its excision.

Extirpation of the rectum for malignant disease I believe to be an ideal operation. Between the plan of the German surgeons of removing the entire rectum up to the sigmoid flexure, and the English surgeons of restricting the operation to a very limited extent, I believe that a middle ground can be established and practiced, based upon a true pathology. It is an axiom in surgery that in operating for cancer, the whole growth must be removed, together with the glands that are involved. Let us take this axiom as our guide in rectal surgery. If the growth extends beyond the point where it is prudent to operate, it is best not to attempt its removal, except, perhaps, for total obstruction, not with any idea of cure. Cripps makes the point that the operation is of doubtful propriety when the disease involves the upper part of the recto-vaginal section, where it is covered with peritoneum. I do not consider this injunction as meaning that it is so dangerous to open the peritoneum, but that this membrane being involved in the disease, renders the operation useless. If, however, there is no fear of the invasion of the peritoneum, an admirable operation is afforded us by Kraske's suggestion, enabling us to remove much of the length of the rectum. The chief argument in favor of his operation is that the entire length of the rectum can be removed, without disturbing the sphincter muscles. In continence of fæces, the result of injury to these muscles, is the one great objection to any other mode of operating.

Kraske's operation is admirably suited to cancerous stricture. It consists in resecting the diseased part, through an opening made at the left side of the sacrum. This operation, of course, is only applicable in a certain class of cases. For instance, if the sigmoid flexure is involved, it would be of no use. If the stricture is low down, it can be divided with a knife. According to his method, the soft parts are divided in the median line from the second sacral vertebra to the anus. The muscular attachments to the sacrum are divided as far as the edge of the opening on the left side. The coccyx is removed, the attachments of the two sacro-sciatic ligaments to the sacrum are cut, and the soft parts drawn to the left side. If still more room is necessary, it may be gained by removing a part of the lower left side of the sacrum. If the bone be divided on a line, beginning on the left edge, at the level of the three posterior sacral foramen, and running in a curve concave to the left, through the lower border of the three posterior sacral foramen and through the fourth to the left lower border of the sacrum, the more important nerves are not injured and the sacral canal is not opened. In this way, the lower part of the rectum, as far as the sigmoid flexure, may be removed. It will be found in this operation, that the dissection is a very difficult one.

Alexander modified this operation, the chief points being that he exsected the coccyx and all of the sacrum necessary to a certain limit. Experimenting with the two, I much prefer Kraske's original operation. I have removed as much as five inches of the rectum, by simply removing the coccyx, making a deep and long dorsal incision, and then practicing a thorough dissection of the gut. The one great object of both of these operations is to keep the sphincter muscles and anus intact. It is true, however, that in some cases the cicatrization is sufficient to establish partial control at least of the actions after the removal of the muscles. This was beautifully illustrated in a case of extirpation which I recently did for cancer. The patient was a man about 60 years of age. The growth extended completely around the gut, beginning at the anus and extending up the rectum for fully five inches. The tissues surrounding the rectum were involved to the extent of three inches. Both sphincters were embraced in the disease. A deep dorsal incision was made through the sphincters and tissues to the sacrum, and brought out over the coccyx. The incision was then made completely around the whole diseased structure, extending from the coccyx, around through the perineum. The gut and sphincters were then carefully dissected out. The vessels were tied as they were uct, drainage

tubes placed, and the wound dressed antiseptically. I did not do as the German surgeons suggest, draw down the end of the rectum to the skin, attach it by a row of sutures, nor did I do as Allingham and Kripps advise, stitch the gut lightly to the skin, and leave the wound to close by granulation. In lieu of both, I left the wound just as I made it, and made no attempt to either bring the mucous membrane down or to stitch it. When the wound had filled up and healed, the cicatrization afforded him protection against any sudden evacuation of the bowels. I am more and more persuaded that where there is a possibility of removing the entire malignant growth from the rectum, excision is far more preferable to colotomy. Excision would remove the disease, thereby having some grounds to hope that it will not reappear. By colotomy we simply palliate the symptoms, leaving the disease in its ravages.

Colotomy.—In a paper read before the Ninth International Medical Congress, which convened in this city in 1887, I took exception to colotomy as a means of treating cancer of the rectum. In that paper I said:

> It is after a careful survey of all the reasons advanced by those who advocate colotomy, in cases of cancer of the rectum, that I am constrained to differ from them and to say that I do not believe that the operation is justified in these cases, except under the rarest circumstances, if at all.

I am still of the same opinion. Too many people are being subjected to this horrible and disgusting operation, that could be benefited equally as much, by simpler means. The operation in itself promises but little. Kelsey says:

> It is the common doctrine taught, that it is to prolong life by the relief of pain, the prevention of obstruction, and in retarding the growth of cancerous disease.

To these statements I cannot give my full approval. First, that to prolong life by the relief of pain I answer, in the majority of cases that have come under my observation, and they have been many, there has not been much pain complained of. Indeed, it has not been a factor. A young lady just sent to me from the South, with a pronounced cancer of the rectum, says she has never suffered any pain. This is simply repeating what many have said to me. If this, then, be the reason assigned for the operation, these cases would be ruled out, and they constitute the majority. But suppose that pain exists, does colotomy relieve it? It may in some cases, but I am certain that in many it does not. As I have said in this article, pain is within the growth itself, by the involvement of the nerves. Surely colotomy could not relieve it. It is likely true that the fæces passing over the growth may irritate it to a certain extent, but my experience has taught me that if the sphincter muscles are not involved, there is but little pain, and if they are involved, scraping the mass out, according to the Germans, will relieve it, equally as well as colotomy would. Again, it must be admitted that after colotomy is done, there is no absolute certainty that some of the fæces will not pass down into the rectum.

Second, that it prevents obstruction. It will be admitted that the greater number of cancers are located in the lower or fixed part of the rectum. It is a recognized fact, too, that total obstruction from fæcal impaction rarely takes place; because it is the disposition of malignant tissue to break down after a certain stage. But suppose it does not, and a stricture, total, if you please, results, to cut through it, divulse it, or resect it, would be better than to do colotomy. The first two will promise equally as much, and the last much more. I have never seen a total obstruction by fæces in a cancerous stricture. I have known total obstruction to occur by the closure of a stricture. We have means of eradicating this by the three methods mentioned. They are much simpler than colotomy, why not do them?

Third, that it retards the growth of cancerous disease. Neither can I subscribe to this proposition. How the establishing of an artificial opening in the loin or groin materially retards the growth in the rectum, I cannot understand. I know that it is used as an argument that the stoppage of the fæces over the growth would, to a degree, stop the growth. This is utter fallacious. The cancer persists in its progressive course of infiltration, ulceration, etc., with as much rapidity after colotomy is performed. Correct statistics of the condition will verify my statement. Admitting that there was truth in the assertion, patients who suffer with cancer to that degree, or extent, requiring colotomy, are not anxious to have their lives prolonged. As a last argument, it is said that colotomy substitutes a painless death for one of great agony. This statement cannot be borne out by facts. Patients who have malignant disease of the rectum usually die of a low and gradual form of peritonitis. I have witnessed a number of such deaths, and they are usually painless. In what way colotomy can substitute a painless death, granting that such a condition exists, I cannot understand. It would appear that they would die very much the same way, whether colotomy is done or not. So I am forced to conclude that neither one nor all of the so-called arguments in favor of doing colotomy, instead of other methods, for cancerous stricture, can be substantiated in fact. If I were asked, when is colotomy justified in cancer of the *rectum*, I would answer, rarely, if at all. But if there be total obstruction of the *sigmoid flexure* from a cancerous mass, and if for any special reason we wish to prolong life a few days, then I believe colotomy would be justifiable. I know that this is a radical view to take of the case, and I also

recognize that the great majority of surgeons will differ from me, but my conclusions have been formed after a care ful investigation of the subject.

But to proceed. Is colotomy to be recommended as a procedure in the treatment of stricture of the rectum? I unhesitatingly answer, yes. Whenever a stricture other than malignant, especially when caused by syphilis, is located in the movable part of the gut, or in the sigmoid flexure, either causing total obstruction or about to cause it, colotomy should be done. If I am asked why, in this instance, and not in cancerous stricture, I would answer, in this we prolong life indefinitely. I see no reason why one should not live many years after the operation done for this condition. A fibrous stricture in the locality named would likely cause death by occlusion, if let alone. It is beyond reach for dilatation, division or excision. There is nothing in the stricture *per se* to cause death, only in the manner mentioned. It acts as a foreign body, causing obstruction. It can never be reabsorbed. It cannot cause death by infection of the body. Having blocked the channel, we open a gate-way above for the escape of fæces, and life is prolonged indefinitely. To do the operation for cancerous stricture, the disease is neither stayed nor cured. The patient dies and the friends ask, why you did so formidable and disgusting an operation, when you knew that death would so soon ensue. To the contrary, in benign or syphilitic strictures, the patient will live to thank you for doing the colotomy. If, then, it is decided to do colotomy, which of the two operations is preferable, the lumbar or extra-peritoneal, or iliac, or intra-peritoneal? I think the anatomical phrase used in designating the two should decide it. An operation extra-peritoneal is certain to be preferred to one that is intra-peritoneal.

I am cognizant of all that is said in regard to the perfect safety of opening the peritoneum under aseptic precautions. Nevertheless, it cannot be gainsaid that it is more dangerous to open the peritoneum than not to open it. I know, too, it is asserted that in doing a lumbar operation, the peritoneum is often opened. This has not been the case in my experience, and in Mr. Bryant's 170 cases of lumbar colotomy, the peritoneal cavity was opened but twice. This disproves the assertion. It is urged in favor of the iliac operation, that by it there can be no possibility of the surgeon mistaking the small intestines, duodenum, or stomach, for the large intestine. Just as well say that it is dangerous to attempt to ligate internal hæmorrhoids, because there is a possibility of including the prostate gland. A surgeon that could not recognize the stomach from the colon, ought not to attempt a colotomy. If the operation is done for the condition that I have named, namely cancerous stricture or obstruction in the sigmoid flexure, the lumbar operation is also preferable for the reason that it is a greater distance from the diseased part and not so apt to be embraced by it.

I have not the time or disposition to argue the pros and cons of this much mooted question. I think Mr. Bryant has met all the objections against the lumbar operation, and I quite agree with him when he says: "Iliac colotomy is not yet proved to be superior to the lumbar operation."

LECTURES.

THE CHEMICAL FACTORS IN THE CAUSATION OF DISEASE.

A Course of Three Lectures delivered at the Post-Graduate Medical School of Chicago, March 26, 27 and 28, 1891.

BY VICTOR C. VAUGHAN, M.D., Ph.D.,
OF ANN ARBOR, MICH.
PROFESSOR OF HYGIENE AND PHYSIOLOGICAL CHEMISTRY AND DIRECTOR OF THE HYGIENIC LABORATORY IN MICHIGAN UNIVERSITY.

(*Continued from page 618.*)

LECTURE II. THE BACTERIAL POISONS OF SOME OF THE INFECTIOUS DISEASES.

An exact classification of the chemical factors in the causation of the infectious diseases can probably not be made at present. We know of two chemically distinct classes, one of which contains substances which combine with acids, forming chemical salts, and which in this respect at least correspond with the inorganic and vegetable bases. The members of this class are designated as ptomaines, a name suggested by the Italian toxicologist, Salmi, and derived from the Greek word πτωμα, meaning a cadaver. A ptomaine may be defined as a chemical compound which is basic in character and which is formed by the action of bacteria on organic matter. On account of the basic properties, in which they resemble the vegetable alkaloids, ptomaines may be called putrefactive alkaloids. They have also been called animal alkaloids, but this is a misnomer, because in the first place some of them are formed in the putrefaction of vegetable matter; and in the second place, the term "animal alkaloid" is more properly restricted to the leucomaines, those basic substances which result from tissue metabolism in the body. While some of the ptomaines are highly poisonous, this is not an essential property, and others are wholly inert. Indeed, the greater number of those which have been isolated up to the present time do not, when employed in single doses, produce any apparently harmful effects. Brieger restricts the term ptomaine to the non-poisonous basic products, and designates the poisonous ones as "toxines." This is a classification, however, which seems to be of questionable utility. It is not always easy to say

just what substances are poisonous and what are not. The poisonous action of a substance depends upon the conditions under which, and the time during which, it is administered. Thirty grains of quinine may be taken by a healthy man during twenty-four hours without any appreciable ill effect, yet few of us would be willing to admit that the administration of this amount of quinine daily for three months would be wise or altogether free from injury. In the same manner the administration of a given quantity of a putrefactive alkaloid to a dog or guinea-pig in a single dose may do no harm, while the daily production of the same substance in the intestine of a man and its absorption continued through weeks and possibly months, may be of marked detriment to the health. We do not as yet know enough about the physiological or toxicological action of the putrefactive alkaloids to render the classification proposed by Brieger worthy of general adoption.

All ptomaines contain nitrogen as an essential part of their basic character. In this they resemble the vegetable alkaloids. Some of them contain oxygen, while others do not. The latter correspond to the volatile vegetable alkaloids, nicotine and coniine, and the former correspond to the fixed alkaloids.

Since all putrefaction is due to the action of bacteria, it follows that all ptomaines result from the growth of these microörganisms. The kind of ptomaine formed will depend upon the individual bacterium engaged in its production, the nature of the material being acted upon, and the condition under which the putrefaction goes on, such as the temperature, amount of oxygen present, and the duration of the process.

A second class of the chemical factors in the causation of the infectious diseases embraces the so-called poisonous albumins. In 1886, Mitchell and Reichert published the results of their investigations into the nature of the venom of serpents, from which they concluded that the poisonous constituents are peptones and globulins. Of the latter they reported the presence of three varieties, each of which is toxical in its action. Although it had been known before this that the injection of any considerable amount of artificially prepared peptone directly into the blood is followed by markedly deleterious effects, this was the first demonstration of the possession of characteristic toxicity by any of the proteids, and from the report of Mitchell and Reichert we may date all modern studies, at least, of the poisonous albumins.

In 1887, Sewall found that tolerance to the venom of the rattle-snake could be established in pigeons by the inoculation of a very small quantity and gradually increasing the amount. In this manner a pigeon was found to bear without ill effect seven times the amount ordinarily fatal to the same animals. The immunity thus obtained was observed to gradually diminish after the discontinuance of the inoculation. However, after five months, while the larger amounts reached in the last injections proved fatal, the ordinarily fatal dose was without effect.

Salmon and Smith protected pigeons against the germ of hog cholera by previous injections with cultures of the same organism which had been sterilized by exposure to a temperature of from 58° to 60° C. for one hour. However, since these cultures contained one or more ptomaines, as well as the poisonous albumins, it could not be, at that time at least, said to which of these constituents the immunity was due.

To Christmas, I believe, is due the credit of first showing the presence of active proteids in bacterial cultures. In 1888, this investigator obtained from cultures of the staphylococcus pyogenes aureus a proteid body which, when injected into the anterior chamber of the eye or under the skin, caused suppuration.

We will now give our attention to the chemical poisons, both the ptomaines and the albumins, of some of the infectious diseases, and in doing this we will illustrate and substantiate the statements made in the preceding lecture.

Anthrax.—The definition of an infectious disease, as we have given it, is well illustrated by the facts which have been learned concerning the causation of anthrax, which has, probably, been more thoroughly studied than any other infectious disease. Kausch taught that this disease has its origin in paralysis of the nerves of respiration. Upon the cause of this paralysis he gave us no information. Delafond thought that anthrax has its origin in the influence of the chemical composition of the soil, affecting the food of animals and leading to abnormal nutrition. The investigations of Gerlach, in 1845, demonstrated the contagious nature of the disease, which was emphasized by Heusinger in 1850 and accepted by Virchow in 1855. However, as early as 1849, Pollender found numerous rod-like microörganisms in the blood of animals with the disease. This observation was confirmed by Brauel, who produced the disease in healthy animals by inoculations with matter taken from a pustule on a sick horse. Attempts were made to ridicule the idea that these germs might be the cause of the disease, and it was said that the bodies seen were only fine shreds of fibrin or blood crystals. Some claimed that the rod-like organisms reported were due to defects in the glass, while others claimed that the defects existed in the eye of the observer, and others still suggested that the defects might be found back of the eye and in the brain. But in 1863, Davaine showed that these little bodies must have some causal relation to the disease, inasmuch as his experiments proved that inoculation of healthy animals with the blood of those sick with anthrax produced the disease only when

taken at a time when the blood contained these organisms. He also demonstrated beyond any question that these rod-like bodies are bacteria, capable of growth and multiplication. The conclusions of this investigator were combated by many; but Pasteur, Koch, Bollinger, De Barry and others studied the morphology and life history of these organisms, and then came the brilliant results of Pasteur and Koch in securing protection against inoculation anthrax by the vaccination of healthy animals with the modified germ and subsequent inoculation with the virulent form. Now, the bacillus anthracis is known in every bacteriological laboratory, and by inoculation with it the disease is communicated at will to susceptible animals. But here the question arose, How do these bacilli produce anthrax? and in answer to this question the various theories which we have mentioned were proposed.

The first successful attempt to study the chemical poisons of anthrax was made by Hoffa, who obtained from pure cultures of the bacillus small quantities of a ptomaine which, when injected under the skin of animals produces the symptoms of the disease and death. This substance causes at first increased respiration and action of the heart, then the respirations become deep, slow and irregular. The temperature falls below the normal; the pupils are dilated, and a bloody diarrhœa sets in. On section the heart is found contracted, the blood dark, and ecchymoses are observed on the pericardium and peritoneum. Hoffa names his poison anthracin.

It has been said that Hoffa's work was the first successful attempt to study the chemical poisons of anthrax. However, his results cannot be considered altogether satisfactory. The small amount of the basic substance which he obtained rendered it highly probable that in the case of a germ so virulent as that of anthrax there must be other chemical poisons produced. This supposition has been confirmed by the labors of Hankin who, in 1889, while at work in Koch's laboratory, prepared from cultures of the bacillus anthracis an albumose which when employed in comparatively large amount proved fatal to animals, but when used in very small quantity gave immunity against subsequent inoculations with the living germ. Unfortunately Hankin does not mention the symptoms induced by toxical doses of this substance. Whether or not the albumose of Hankin contains *in statu nascendi* the base of Hoffa, and owes it poisonous properties to the same, has not been determined. There is yet a promising field for research in the chemistry of this disease.

Asiatic Cholera.—There are good reasons, apart from experimental evidence, for believing that the comma bacillus of Koch produces its ill effects by the elaboration of chemical poisons. This germ is not a blood parasite. It grows only in the intestine, and the symptoms of the disease and death must result from the absorption of its poisonous products. In confirmation of this statement, experiment has shown that this is one of the most active, chemically, of all known pathogenic germs.

In the first place, Bitter has shown that the comma bacillus produces in meat-peptone cultures a peptonizing ferment, which remains active after the organism has been destroyed. Like similar chemical ferments it converts an indefinite amount of coagulated albumin into peptone. It is more active in alkaline, than in acid solutions, thus resembling pancreatin more than pepsin. This resemblance to pancreatin is further demonstrated by the fact that its activity is increased by the presence of certain chemicals, such as sodium carbonate and sodium salicylate. That a diastatic ferment is also produced by the growth of the bacillus was indicated, in the experiments of Bitter, by the development of an acid in nutrient solutions containing starch paste. However, all attempts to isolate the diastatic ferment were unsuccessful. A temperature of 60° C. destroys or greatly decreases the activity of ptyalin, and this seems to be true also of the diastatic ferment produced by the comma bacillus. But the formation of an acid from the starch presupposes that the starch is first converted into a soluble form.

Rietsch believes that the destructive changes observed in the intestines in cholera are due to the action of the peptonizing ferment.

Cantani injected sterilized cultures of the comma bacillus into the peritoneal cavities of small dogs and observed after from one-quarter to one-half hour the following symptoms: Great weakness, tremor of the muscles, drooping of the head, prostration, convulsive contractions of the posterior extremities, repeated vomiting and cold head and extremities. After two hours these symptoms began to abate, and after twenty-four hours recovery seemed complete. Control experiments with the same amounts of uninfected beef-tea were made with negative results. The cultures used were three days old when sterilized. Older cultures seemed less poisonous and a high or prolonged heat in sterilization decreased the toxicity of the fluid. From these facts Cantani concluded that the poisonous principle is volatile, but the effect of high or prolonged heat in diminishing the toxicity was more probably due to its destructive effect on the poisonous proteids.

Brieger succeeded in isolating from cultures of the comma bacillus in meat broth two basic substances which he considers the specific poisons of cholera. One of these, found in the mercuric chloride precipitate, is a diamine, resembling trimethylenediamine. It produces muscular tremor and heavy cramps. In the mercury filtrate was found another poison, which, in mice, produced a lethargic condition; the respiration and heart's

action became slow, and the temperature sank, so that the animal felt cold. In some instances there was a bloody diarrhœa.

Later Brieger and Fränkel have obtained a cholera-albumin which proves fatal to guinea-pigs after two or three days, but is without effect upon rabbits.

More recently Winter and Lesage treat a bouillon culture of the cholera germ with sulphuric acid, dissolve the precipitate in an alkaline medium, reprecipitate with acid, and redissolve in ether, which on evaporation leaves oily drops which, on cooling, form a yellow mass of the appearance of a fat. This substance is insoluble in water and acids, soluble in alkalies and ether. It melts at 50° C., and does not lose its virulence on being boiled with alcohol rendered feebly alkaline. The virulence of a culture and the amount of this substance contained therein are in direct proportion to each other.

Small doses of this substance (one milligram to 100 grams of body weight of the animal) in feebly alkaline solution introduced into the stomachs of guinea-pigs cause, as a rule, within from four to six hours, a chill, and death after twenty-four hours. With larger doses the temperature falls after from one-half to one hour, and death results within from twelve to twenty hours. Smaller doses cause a less marked reaction and the animal recovers within twenty-four hours. If killed within this time the animal shows a choleraic condition. Rabbits succumb only after repeated subcutaneous injections. The substance can be extracted from the muscles, liver, kidneys and urine of the poisoned animals. This substance can also be obtained from cultures of a cholera infantum germ. The fact that this poison belongs neither to the ptomaines or albumins is of interest.

Cholera Infantum.—The fact that Booker, Escherich and other investigators have failed to find any one germ constantly present in the bowels or stools of children suffering from summer diarrhœa was mentioned in the preceding lecture. Notwithstanding this, the fact that chemical poisons are concerned in the production of these diarrhœas has been abundantly demonstrated. In the first place the symptoms of the choleraic form of these diarrhœas are so very similar to those induced by a number of gastro-intestinal irritants, which at the same time depress the nerve centres, that the suggestion that the disease might be due to chemical poisons has been favorably received by the profession. The poisonous substance first found by the writer in cheese and afterwards in ice-cream, milk and various milk preparations, and which is known as tyrotoxicon, induces symptoms which cannot be distinguished from those of choleriform diarrhœa in infants. Moreover, the post-mortem appearances are very much alike, if not identical, and the poison has been found in a sample of milk, a part of which had been given to a child not more than two hours before the first symptoms of a violent attack of the disease manifested themselves.

Tyrotoxicon however, is only one of the poisons which may appear in milk and milk products, or may be formed in the intestines of the infant and cause an exhausting diarrhœa. I have recently isolated from samples of poisonous cheese, in which tyrotoxicon could not be detected, a poisonous albumin.

I have also isolated from pure cultures of the germs, x, a, and A of Booker's list of summer diarrhœa microörganisms, three poisonous bodies, small quantities of which injected under the skin of kittens and puppies cause retching, vomiting, purging, collapse and death. Ten milligrams of the dried proteid from "a" killed a large guinea-pig within twelve hours, and a smaller amount proved fatal, in other experiments, after a longer time. These poisons differ from one another, more or less, in their physical and chemical properties, but all produce the above mentioned symptoms. It may be that a more exact study of the effects induced will enable us to distinguish, in this way, one from the other. Evidently, here are three germs differing from one another morphologically sufficiently to be classified as different species, but belonging chemically and toxicologically to the same group. How many more of the long list of bacteria found by Booker, Escherich and others in the study of these diseases remains to be ascertained. There is no reason, however, for supposing that in taking three of these at random, I have hit upon the only toxicogenic ones.

The fact that Baginsky's cholera infantum germ produces a poisonous albumin was referred to in the first lecture; also the formation of a choleraic poison by the germ of Lesage has been mentioned in discussing the chemical poisons of Asiatic cholera.

The prevention of the summer diarrhœas of infancy becomes a more difficult task than those who believe that a single microörganism is concerned in their causation admit. All bacteria which are capable of growth and reproduction within the intestines of the infant, and of the production of chemical poisons during their development, must be excluded. The treatment of the same diseases will not be wholly successful until we know how to antidote these poisons. To accomplish these ends will probably require many years of research and observation by our profession. However, if this be the right line, let us follow it, realizing that our successors will profit by our mistakes, and render our partial successes more successful.

Diphtheria.—Roux and Tersin rendered cultures of Löffler's bacillus of diphtheria sterile by filtration through porcelain, and injected thirty-five cubic centimeters of the filtrate into the ab-

dominal cavity of guinea-pigs. No immediate effects were observed, but after two or three days, the animals refused to eat, passed bloody urine, showed muscular weakness in the posterior extremities, manifested marked irregularity in respiration and died after from five to six days. Section showed marked renal hyperæmia and a serous exudation in the pleura. In cases of less acute intoxication, paralysis of certain groups of muscles become apparent.

The cultures first employed were seven days old. Older cultures (six weeks) contain more of the poison, and the symptoms appear within a few hours after the injection. In cultures especially rich in the poison, a small amount (from 0.2 to 2.0 cc.) injected under the skin suffice to induce the symptoms. The place of injection becomes œdematous, the respiration rapid, and death results as quickly as after inoculation with the germ. Mice and rats are markedly insusceptible, but succumb to large doses.

The action of this poison was found to be much lessened by continued exposure to a temperature of only 58°. These investigators believed that this poison belonged to the group of enzymes.

Brieger and Fränkel have continued the study of the chemical poison of the Löffler bacillus. These investigators find that this poison can be heated to 50° in the presence of an excess of hydrochloric acid without being destroyed, and they conclude from this that the substance cannot be an enzyme or ferment. The poison is precipitated from the culture, after sterilization by filtration through porcelain, by absolute alcohol, slightly acidified with acetic acid. It forms a snow-white, amorphous mass. It stands very closely related to serum-albumin. It may also be precipitated by saturation with ammonium sulphate, and can be purified by removing the sulphate by dialysis. This reaction distinguishes it from the peptones. In small doses (two and one-half milligrams per kilogram body weight), injected into the blood-vessels, it causes death, not like other poisons, immediately, but after days, weeks or months, with the formation of abscesses and necrotic tissue at the point of injection, with progressive emaciation and paralysis. Brieger and Fränkel propose for this and similar bodies the name "toxalbumin." As was stated in the first lecture, these studies leave no room for us to doubt that the Löffler bacillus is a cause of diphtheria. The chemical poisons of Prudden's streptococcus and of the germ found by Klein in the diptheria of some of the lower animals, have not been investigated.

Suppuration.—The fact that a pus-producing chemical poison was found in cultures of the staphylococcus pyogenes aureus has been already noted. In addition much interesting work on the poisons of suppuration has been done. That the ptomaines, cadaverine and putrescine, of Brieger, when injected under the skin in sterile solutions, cause abscesses, has been known for some years. It has been shown that sterilized cultures of a number of different germs produce suppuration. Thus, Büchner showed that sterilized cultures of the bacillus of Friedlander contains pyogenetic substances, and when injected into man in very dilute solution produce an elevation of temperature. In view of the fact that so much has been said about the wonderful effect of small doses of the so-called cure for tuberculosis proposed by Koch, the following account of an experiment made upon himself and reported by Büchner early in 1890, may be of interest: Five tenths of a cubic centimeter of a sterilized very dilute emulsion of Friedlander's bacillus was injected under the skin of the forearm, after it had been found that 500 times this quantity, reckoned per kilogram of body weight, caused no local irritation or suppuration in a rabbit. One and one-half hour after the injection sharp pain extended along the arm to the axilla. One-half hour later there was a chill followed by a fever which reached a maximum of 36.6° in the axilla five hours after the injection. At the same time the pulse arose to 108. On the following day there were well marked erysipelatous swelling and redness extending over an area larger than a saucer about the place of inoculation, and there was marked redness along the lymphatics of the arm. On the third day all these symptoms disappeared.

Büchner has shown that the cells of seventeen different species of bacteria contain pyogenetic substances. The cells may be boiled for hours, their substance dissolved in dilute alkali and precipitated by acid without losing its pyogenetic properties. It should be understood, however, in this case that the active substance is contained within the bacterial cell, and this leads us to distinguish between the chemical poisons which arise from bacterial activity and are found in solution in cultures, and those which reside within the cell and form an integral part of the same. Büchner finds this active substance within the cell to be proteid in character. It gives the biuret, Millon's and xanthoprotein reactions. Before the substance can cause suppuration within the body it is most likely necessary for the bacterial cells to suffer disintegration. It is hardly necessary to add that the pus formed by this bacterial proteid is germ-free.

Typhoid Fever.—The poisons of this disease are both basic and proteid, though those belonging to the latter group seem to be the more virulent.

In 1885 Brieger obtained from pure cultures of the Eberth bacillus a ptomaine, which produced in guinea-pigs a slight flow of the saliva, increased frequency of respiration, dilatation of the pupils, profuse diarrhœa, paralysis and death within twenty-four to forty-eight hours. Post-mortem examination showed the heart in systole, the

lungs hyperæmic and the intestines contracted and pale. Brieger was at first inclined to regard this as the specific poison of typhoid fever and named it "typhotoxine." However, he obtained with this substance no elevation of temperature.

In 1887 Novy and the writer obtained from cultures of a germ found in drinking water which had been the supply of many persons who had typhoid fever, an extract which, when injected under the skin of cats, caused an elevation in the temperature of from two to four and one-half degrees.

In 1889 the writer obtained, from mixed cultures made from typhoid stools, a small amount of a basic substance which caused, in the lower animals, an elevation of temperature accompanied by profuse diarrhœa.

Brieger and Fränkel obtained, from cultures of the Eberth bacillus, a poisonous albumin, but have reported no particulars concerning its action. The writer has two germs, obtained from drinking water supposed to have caused typhoid fever, and which, after having been kept for some days at a fever temperature, give all the reactions which have hitherto been supposed to be characteristic of the Eberth bacillus. From pure cultures of each of these he has isolated highly poisonous proteids, which, in dogs, cause a chill, followed by marked elevation of temperature. These poisons are soluble in water, from which they are not precipitated by heat or nitric acid, singly or combined. They are precipitated by saturating the aqueous solution with ammonium sulphate, and are not, therefore, peptones. They are not precipitated by carbonic acid gas, or by sodium sulphate to saturation, and this excludes them from the class of globulins. The aqueous solutions respond to the xanthroprotein and biuret reactions. Chemically I can so far distinguish no difference between the poisonous albumins produced by the two germs, but physiologically there is a marked difference. That from bacillus A produces paralysis of the voluntary muscles, which is preceded in some animals by most violent convulsive movements. With the proteid of bacillus B neither the convulsions nor the muscular paralysis has been observed. The animal seems to die from gradual failure of the heart. However, these poisons demand a much more extensive and exhaustive study than has yet been made. It will be seen from what has been said, that the chemistry of typhoid fever promises to be both fertile and interesting in results.

Tetanus.—Brieger has obtained from the mixed cultures of the germs of Nicolaier and Rosenbach, four poisonous substances. The first, tetanine, which rapidly decomposes in acid solutions, but is stable in the presence of free alkali, produces tetanus in mice when injected in quantities of only a few milligrams. The second, tetanotoxine, produces tremor, followed by severe convulsions. The third, to which no name has been given, causes tetanus, accompanied by a free flow of the saliva and tears. The fourth, spasmotoxine, produces heavy convulsions.

Later, the same investigator extracted tetanine from an amputated arm of a man with tetanus. This is of special importance, since it answers the assertion made by Baumgarten, that there is no proof that the poisons which have been obtained from artificial cultures, are formed within the body of man.

From pure cultures of the tetanus bacillus of Kitasato, Brieger and Fränkel have isolated a poisonous proteid, and with cultures of another bacillus, Tizzoni and Cantani have been equally successful. These investigations leave us without cause to doubt that the convulsions of traumatic tetanus are due to one or more spinal poisons, and the causal relation of a given bacillus is determined not by its morphological characteristics, but by its capability of producing a chemical substance which has a definite toxicological effect.

Tuberculosis.—Whatever may be the ultimate verdict concerning the curative properties of Koch's tuberculin, its employment has made us familiar with the action of the chemical products of the bacillus tuberculosis on man. Unfortunately, Koch has given us but little information concerning the nature of his tuberculin, and the little which he has given us has been to some extent misleading. I would not imply that he has intentionally been misleading. Indeed, I believe that such was not his intention. He speaks of the agent as an extract of a pure culture of the bacillus tuberculosis with 50 per cent. glycerine. Now, chemists who have been at work with bacterial poisons, distinguish between bacterial products and the contents of the bacterial cells. One would infer from Koch's statements that tuberculin is prepared by extracting the bacterial cells with 50 per cent. glycerine, and that the bacterial products are not present. But, as has been shown by Hueppe and Schall, the proteids of the cells of the bacillus tuberculosis cannot be extracted with 50 per cent. glycerine. Moreover, the same investigators have prepared a fluid identical in physical properties, in chemical reactions, and in its effects on animals, with Koch's fluid, by each of the three following methods:

1. Cultures of the bacillus are filtered, sterilized by heat and concentrated.

2. The supernatant, fluid portion of the culture is decanted from the mass of germs at the bottom of the flask, and then concentrated.

3. The culture is freed from germs by filtration through a Chamberland filter and concentrated.

These fluids contain, 1, the constituents of the nutritive medium which have not been altered by the growth of the germ, such as glycerine, albu-

mins, albumoses and peptones; 2, the bacterial products, which may possibly belong to the ptomaines, the bacterial albumins or albumoses and bacterial ferments; and 3, any constituents of dead, broken-down bacilli which may have passed into solution. To which of these constituents the action of the fluid is due has not been positively determined. However, from the similarity in the action of this fluid with that of the bacterial products of other germs, we seem justified in assuming that these constitute the active principle.

As early as 1888, Hammerschlag found a poisonous proteid among the products of the growth of this germ.

Recently Zuelzer has reported the isolation of a poisonous ptomaine from agar cultures of the bacillus tuberculosis. He says that the injection of 1 centigram or less of this substance subcutaneously in rabbits or guinea pigs causes, after from three to five minutes, increased frequency of respiration (to 180 per minute?) and an elevation of temperature of from 0.5° to 1°. He also reports marked protrusio bulbi as a constant symptom; the eyes become very bright and the pupils are dilated. From two to three centigrams suffice to kill rabbits, death occurring in from two to four days. The place of injection is reddened and hæmorrhagic spots are formed in the mucous membrane of the stomach and small intestines. In two instances from 15 to 20 cc. of clear fluid were found in the peritoneal cavity.

(*To be concluded.*)

MEDICAL PROGRESS.

Therapeutics and Pharmacology.

TREATMENT OF BED-SORE.—BILLROTH recommends that the part should be bathed with vinegar or lemon juice as soon as erythema appears. Excoriations should be touched with nitrate of silver and the part dressed with simple cerate, or soap plaster spread upon linen or soft leather When gangrene occurs the surface should be covered with a disinfectant, such as chlorinated water. Carbolic acid should be avoided, because of its liability to cause poisoning. Energetic internal measures should be employed, to strengthen and build up the system, such as wine, acid drinks, quinine and musk.—*L'Union Medicale.*

Medicine.

DETERMINATION OF THE LOWER BORDER OF THE LIVER.—VERSTRALTEN (*Centralblatt für Klin. Med.*) points out how difficult it is in many cases to outline the surface, and particularly to determine the exact position of its lower border. This he thinks is easier by auscultation than by any other method; taking advantage of the excellent conducting power of the liver, the stethoscope is placed over the heart and then gradually moved downward and to the right. The point at which the heart sounds are no longer heard marks the lower border of the organ. A source of error is contracted abdominal walls; they should be relaxed as far as possible, if necessary, by placing the patient in a sitting posture.

Surgery.

TEN YEARS' EXPERIENCE OF GASTRIC SURGERY.—DR. TORRAS has made a study of the statistics of the resection of the pylorus during the years from 1880 to 1890. From this it appears that in 1880 there was one extirpation followed by death. In 1881, out of 20 in whom the section of the pylorus was performed, there were 15 deaths and 5 cures. In 1882, the numbers stood, as out of 16 cases: 13 deaths and 3 cures; in 1883, 13 cases, 7 deaths and 6 cures; in 1884, 5 deaths and 3 cures; in 1885, 5 cases, 3 deaths and 2 cures; in 1886, 3 cases, 1 death and 2 cures; in 1887, 2 cases, 2 cures; in 1888, 1 case, 1 cure; in 1889, 1 case, 1 cure; in 1890, 2 cases, 2 cures.

In reviewing these statistics Dr. Torras observes that the surgical enthusiasm which led a man so eminent as Billroth to sanction this operation has evidently decreased, till, in 1889, only one operation of the kind was performed, and two in 1890. This proves that the results have not corresponded to the hopes conceived, and that such an operation is very rarely indicated in cancer of the stomach. Dr. Torras details a case of exploratory laparotomy performed by Dr. Fargas in a woman of 52, who had for several months suffered from vomiting and pain in the gastric region. In this situation a moveable tumor could be felt, about the size of an orange, which was supposed to be malignant, and to be attached to the gastro-colic epiploon. On opening the abdominal wall, it was found to be a carcinoma of the head of the pancreas, adhering to the duodenum and pylorus. Under these conditions, Dr. Fargas did not venture to attempt removal, but confined himself to cauterize with the thermocautery some points of the bleeding surface of the tumor, and to close the incision with sutures. The outer wound healed in ten days, and the patient felt better for a month after, when she died in a cachetic condition.

Though most of those stated to be cured by the operation have survived above five years, yet none of them can be regarded as really out of danger from the disease. Dr. Torras would confine the operation of gastrotomy to cases in which digital or instrumental dilatation might be practiced for simple stenosis of the pylorus, or for contraction of the orifice from cicatrices or fibrous tumors, or for the extraction of foreign bodies from the stomach.

THE
Journal of the American Medical Association
PUBLISHED WEEKLY.

SUBSCRIPTION PRICE, INCLUDING POSTAGE.
PER ANNUM, IN ADVANCE $5.00
SINGLE COPIES . . . . . 10 CENTS.

Subscription may begin at any time. The safest mode of remittance is by postal or express money order, drawn to the order of THE JOURNAL. When neither is accessible, remittances may be made at the risk of the publishers, by forwarding in REGISTERED letters.

Address
JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION,
No. 68 WABASH AVE.,
CHICAGO, ILLINOIS.

All members of the Association should send their Annual *Dues* to the *Treasurer*, Richard J. Dunglison, M.D., Lock Box 1274, Philadelphia, Pa.

LONDON OFFICE, 57 AND 59 LUDGATE HILL.

SATURDAY, MAY 9, 1891.

SURGICAL TREATMENT OF GENERAL PARALYSIS.

The possibility of curing general paralysis by trephining the skull is again coming into prominence. Two years ago DR. SHAW, of London, performed the first operation of this kind, which was practically a success. He reasoned that all the symptoms in general paralysis seemed to indicate an irritative inflammatory process in the upper layers of the convolutions. An operation would produce an alteration in the morbid process; on the theory of nerve stretching he proposed to give the brain at this point more space to expand, and relieve itself of the increased arterial pressure, which the sphygmograph indicated as one of the early conditions of this disease. He hoped by relieving the fluid pressure to stimulate some new nutritive processes.

The patient upon whom the operation was performed was in the second stage of the disease. He had delusions of grandeur, affection of speech, exaggerated reflexes, unsteady gait and retention of urine. Convulsive attacks followed, with periods of loss of sensation, difficulty in swallowing and talking, and rapidly approaching dementia. The trephine was applied on the right side of the skull, over the central sulcus, two inches outside of the longitudinal fissure. The patient improved to such an extent that he was discharged as recovered.

From post-mortem appearances in such cases both atrophy of the brain and increase of brain fluids are found. It is not known whether this condition comes from pressure of brain fluid or whether the fluid collects on account of brain shrinking. The extreme bulging of the dura in this case seemed to show that the first theory was correct. By removing a portion of the bone, and corresponding meninges of the brain, not only would the existing fluid be let out, but a permanent contact would be established between the secreted fluid and the under surface of the scalp-flap, to the absorbent of which was entrusted its future removal.

The second case was operated on by DR. BATTY TUKE. It was farther advanced and more hopeless. A large button of bone was taken out, and the patient was more sane and improved in every way for some time; finally the old symptoms returned, and he was taken to DR. CLOUSTON'S asylum at Edinburgh, and at present he is hopelessly demented. DRS. TUKE and CLOUSTON both considered that the operation was fully justified, and that the modifications and improvements in the symptoms were hopeful signs of what might be expected in more recent cases.

Several criticisms have been made on the theories of this operation. It is asserted that the paralysis, loss of power of talking, attacks of stupidity, etc., are not due to fluid pressure, and that the excess of fluid is only a secondary and compensating process. Also that holes in the skull cannot relieve the pressure on the convolutions. TUKE asserts that obstructed lymph makes its way very imperfectly by natural channels to the pia-matral space and becomes diffused through the tissues, injuring and displacing cell and fibre, and impairing their functional activity, while the operation, by permitting a healthy action of the lymphatics and blood-vessels, stays the process of sclerosis.

The criticism of this view is that only in the second stage of this disease is there any extraordinary development of the lymph connective system of the brain, with degeneration and disappearance of nerve elements, the axis cylinders of which are denuded; that the first stage is one of true degeneration due to acute interstitial changes, that are similar to senile atrophy. That the excess of fluid in the second stage is of secondary importance, and is the only stage when an operation can be expected to be of any value on this theory, while in fact the case has passed all possibility of cure in this stage. The first stage is the only period that any operation can be justi-

fied, but at this time no lymph has been formed, and no benefit can be expected on the pressure theory. The critics affirm that any change which may follow this operation is due to a natural remission in the disease.

From these theories and criticisms there are many reasons for expecting some startling developments along this new line of brain surgery. The general hopelessness of these cases, and inability of remedies to even stay the progress of the disease, would seem to justify the surgeon in experiments, which cannot alter the final issue of the case, under any circumstances. The reckless use of powerful remedies experimentally and theoretically is common in nearly all cases, and the surgeon is equally justified in attempting means that give promise of relief. These two cases will arouse new interest in this disease, and suggest theories of pathology and treatment that would have been impossible a few years ago.

THE VALUE OF KOCH'S TUBERCULIN.

The therapeutical results obtained by tuberculin may be summarized in two statements: 1. With the exception of certain forms to be specified hereafter, *the agent exerts a specific effect on all tubercular lesions, by starting an inflammatory process in and around the tubercular tissue, which tends to cure the disease.* 2. *This reparative change has* NOT *proceeded up to the actual cure in the majority of cases hitherto treated.* The exceptions not influenced by tuberculin are old lesions in a state of caseation, a fact already pointed out by KOCH in his first communication, but also fresh miliary eruptions, attention to which latter important observation has been called by VIRCHOW and other pathologists. The inability of tuberculin to affect fresh miliary tubercles or to prevent their dissemination, and the frequent cessation of its curative influence before the infiltration has completely disappeared, are at present the main obstacles to success with this treatment. But in order to review what the agent can do and has done, it is necessary to consider the various tubercular localizations separately.

In *lupus*, with scarcely an exception, the agent has produced an improvement which, competent observers claim, cannot be obtained by any other means within the same time. This improvement can proceed up to absolute cure of the disease, at least for the time being. Thus, of the later authors, DOUTRELEPONT states that of thirty-one cases of lupus four were completely cured, and all the others were in a state of marked improvement. But in the majority of cases the improvement ceases before the entire patch is healed, while in others where a smooth cicatrix has resulted, nodules can still be felt under the skin and —on the cessation of the treatment—these give rise to fresh manifestations of the disease. Yet most of the writers admit that they could not have benefited their patients to the same extent within the same time by any other means.

In tuberculosis of the mucous membranes and of the larynx especially, perhaps the most brilliant results have been accomplished with tuberculin. Numerous cases have been placed on record, in which the ulcers healed completely under the eyes of the observer, even when the accompanying pulmonary lesion was not benefited to the same extent. It has often been noticed that on mucous surfaces, and especially in the larynx, fresh nodules and even minute ulcers appear in the vicinity of the preëxisting lesion under tuberculin treatment. In one of such instances FLATAU has proven by histological examination (presence of bacilli) that the nodule was a true tubercle. It is an open question whether these "new" lesions are the result of preëxisting bacilli, the presence of which is made manifest by the tuberculin influence, or whether they indicate really a dissemination of the virus. At any rate, they disappear readily under the continuance of the treatment, differing in this respect from fresh eruptions in internal organs. A few instances of laryngeal tuberculosis (and also diseases of other mucous surfaces) have been reported without benefit from tuberculin, but as a rule this agent has cured the tubercular localization in these parts with a rapidity not equaled by any other means at our command. It is not possible to prove its relative efficacy by statistics as yet. In the official report to the German Government on the efficacy of KOCH'S remedy 108 cases of laryngeal tuberculosis are mentioned, of which seventy-one had been more or less improved. But this report includes only experiences of from two to six weeks, and that during the time when most observers were only learning the mode of employment of the substance. More definite

statements can be obtained from the separate publications in periodic literature, but in most cases the number of cases is not fully stated.

In *phthisis* satisfactory results have been obtained, as a rule, only in *incipient* cases, *i. e.*, of short duration, weeks or months, infiltration limited in extent, no cavities and no continuous fever. KOCH himself cured his first two patients, and repeated examination has confirmed their continued good health during the last three to four months. The majority of incipient consumptives are reported as improved, at least subjectively, by most of the authors. Gain in strength, cessation of night-sweats and of evening rise of temperature, and diminution of cough, indicate a favorable change which, under the observation of GUTTMANN, is said to have occurred in 41 of 51 cases of incipient phthisis. The improving patients do not always gain in weight, although this often occurs. Evidently the reparative influence is counterbalanced by the increased tissue consumption during the febrile reaction, especially if the latter be intense. But a consumptive cannot be said to be cured until all cough has ceased, and no sputum containing bacilli can be obtained. A perfect cure does not necessarily imply that all abnormal percussion and auscultatory sounds have disappeared. For where there was any extensive consolidation of lung tissue we can neither expect normal breathing sound nor resonance of normal lung tissue. There will always remain a shrinkage of the involved part of the lung. But applying all reasonable criticism, we can still say that not a few cases of completely cured incipient phthisis are now on record. Perhaps the most favorable figures are claimed by STRICKER, who speaks of eight absolute cures among nine young soldiers with incipient phthisis, whilst amongst forty-seven soldiers, whose pulmonary trouble was more advanced, four could be considered completely well at the end of treatment. While many observers speak nearly as highly as STRICKER of their experience in incipient phthisis, especially in private and in sanitarium practice, others like SCHULTZE report but little success, but such pessimistic reports are decidedly in the minority. Yet it must be admitted that a few instances even of beginning phthisis get worse under the treatment, whether in consequence of or notwithstanding tuberculin, is an open question. Any continuous febrile movement produced by tuberculin can be considered a contraindication against its further use.

The *more advanced the phthisis* the less can be expected of KOCH'S remedy. Cases of diffuse infiltration, extensive cavities, or rapid course towards the worse have but rarely been arrested in their progress, and occasionally been injured by tuberculin. Still, with some discretion in their selection, and the cautious use of minute initial doses, they need not all be excluded absolutely from the treatment, for in a fair number of instances at least subjective amelioration has been noted. As KOCH suggested in his first note, the influence of tuberculin should be aided by all measures known to benefit such patients. The most promising results have been obtained by SONNENBURG in KOCH'S hospital wards by the surgical treatment of cavities combined with tuberculin injections. Only those cases were selected in which the cavities were limited to the upper lobe of one lung; the patients being yet in a state of moderate vigor. After cutting down to the fascia with the resection of a piece of the first rib, a trocar was plunged into the cavity, the wound enlarged with the thermo cautery and a strip of iodoform gauze inserted. SONNENBURG considers the operation easy and comparatively safe when there are adhesions between the two layers of pleura, and but little more serious when the pleural space still exists over the region of the cavity. He reported five cases, one of which was cured inside of four weeks while two more were on the road to recovery. One died, but from the disease, and not in consequence of the operation, while the fifth had but been operated upon just before publication. Others have since repeated the operation, but their results are not yet known.

In *pleurisy of tubercular origin* a rapid absorption of the effusion has been observed in most instances, but the pulmonary lesion itself has not been invariably benefited, its course being influenced in about the same manner and with the variable results as observed in similar instances without pleural complication.

MULTIPLE BIRTHS. — An English physician writes to the *British Medical Journal* of a woman who had twins five times, triplets once, and single children four times.

"IN SIMPLICI SALUS."

HIPPOCRATES proclaimed that "accurate observation of facts, and correct generalization from them, forms the only rational basis of medicine," and so we are taught to-day. To discover truth in science, the most learned will admit, is very often difficult, but in no science is it more difficult than in medicine. Independent of the common defects of medical evidence, our self-interest, our self-esteem, our prejudices, our likes and dislikes, and not infrequently our ignorance, only too often hide the truth from our view, and we ascribe too much to art, and too little to the operations of nature. Thus the mass of testimony is most with art, and, although we believe we are right in our reasoning, we only pursue the old, time-honored course that has been instilled into our minds through training and education. The best and safest practitioner is he who knows when to abstain from acting as well as when to act; in other words, who has learned when and to what extent the case can be left to the salutary processes of nature. The tendency to recovery which manifests itself under different modes of treatment, and even in spite of opposite modes, has induced in some minds a degree of skepticism as to the utility of any remedies. That the opposite error to that of mischievous or meddlesome activity may likewise be easily carried far is at once apparent. It does not follow because the majority of diseases, such as continued fevers and acute affections generally get well, with or without the administration of medicine, that therefore the disease should be abandoned to what Cullen calls the "*vis medicatrix naturæ*."

A knowledge of the circumstances upon which health depends is one of the most important parts of the moral and intellectual education of the true physician. The essentials necessary to the attainment of health are:

1. The inheritance of a healthy constitution.
2. Pure atmosphere and water.
3. Wholesome food in quantity and quality.
4. Freedom from contagious and infectious diseases.

Either of these primitive essentials of health is controlled but little, if any, by the individual efforts of man. To medical science solely must mankind look for the foundation rock from whence the principles governing these essentials of nature's law are to be revealed by virtue of intelligent, progressive, active, zealous, and truly conscientious physicians, who will, sooner or later, succeed in educating the legislator to understand and realize the fact paramount, that only through the State or municipal government can we ever hope to see mankind enjoy these blessings. Nature puts into our hands the means of preserving health, and this gift involves responsibility. Health will be counted among those talents for the use of which we are to answer to our Creator, and it is our duty to become fully acquainted with the laws which regulate and govern it.

Experience teaches that disease, as well as health, is controlled by nature, that her laws must be consulted if we would practice successfully. But alas! how often do we find—even in this most enlightened age of science—the truth as expressed by the late PROF. CHAPMAN, "That many physicians are given to profound thought, and possess extensive knowledge, united with sterling honesty, being by nature endowed with the highest order of talents, and yet be wanting in good common sense." The most experienced, close observing, earnest searchers after truth in nature's operations, most skillful and best physicians, hesitate above all things to give large quantities of medicine, and proclaim the best way to help the invalid to health is simply to "assist nature." The writings of Drs. Benjamin Rush, Shippen, Chapman, Radcliffe, Bostwich, Dumoulin, and Oliver Wendell Holmes, Sir Astley Cooper, Sir Wm. Gull, and numerous other shining lights are too well known to need reiteration, but are simply called to mind as evidence that the more matured minds in the profession are guided by the light of nature's *in simplici salus*.

EDITORIAL NOTES.

ENDOCARDITIS DURING MEASLES.—Mr. J. H. Hutchinson submitted a contribution to the Royal Medical and Chirurgical Society of London, at its meeting of April 14, 1891, in which a connection was held to subsist between measles and endocarditis, four cases being cited in which the heart affection developed during the course of the acute exanthematous disease. The opinions of the medical gentlemen present were divided on the subject, some declaring that heretofore

unexplainable cases might find solution through the possible influence of the specific poisons producing the acute febrile diseases of childhood. The cases presented by N. W. Hutchinson, though few, were yet sustained, in a measure, by the post-mortem appearances of other cases with like characteristics.

This connection—or, as we should more readily believe, coincidence—has been noted before. We quote Garrod, as follows: "Dr. Sansom is of opinion that the importance of measles as a predisposing cause of endocarditis has not been sufficiently taken into account, and he is inclined to assign to that fever a share in the production of articular rheumatism also. He has, moreover, recorded a case in which chorea, associated with pericarditis and endocarditis, developed during convalescence from an attack of measles."

Second Childhood.—This is often spoken of as a condition into which those of advanced life enter. The mind, disposition, and actions become child-like, until finally the evening of existence becomes no less lightsome and blank to sterner realities than maintained at the dawn of life. However that may be, we have in medicine occasional evidences which, if they do not point to a secondary physical childhood—so to speak—yet are sufficiently interesting to have note. Among these points of interest may be mentioned a case of scarlet fever occurring recently in a woman 59 years of age, and a case of whooping-cough in a man aged 84.

A Praiseworthy Relief Organization.—The Society for Relief of Widows and Orphans of Medical Men (British), of which Sir James Paget is President, is active in the work towards which its title points. Examples of signal benefit, in the operations of such societies, are not at all few, and, like all such acts of honorable charity, the result is of a two-fold nature.

Inebriatism a Disease.—As emphasizing the trend of professional opinion in the direction of the morbidity of inebriatism may be mentioned the proposals embodied in the President's Address before the Society for the Study of Inebriety, of London, as follows:

1. "In all criminal trials in which the alleged criminal act has been committed by the accused when under the influence of liquor, or has been committed by an inebriate, there should be an investigation into the previous medical history of the prisoner. There should also be an inquiry into the family history, so as to elucidate the heredity with special reference to inebriety, insanity, and other neurotic affections, syphilis and gout. This twofold inquiry should be entrusted either to a medical expert, or to a mixed committee composed of a legal and medical expert acting conjointly. The object of this investigation is to ascertain how far the accused has been cognizant of his alleged criminal offence, as to whether he was competent to discern its consequences, and as to whether, if so cognizant and competent, he was able to resist the criminal impulse. Such an expert inquiry should be provided for the accused, whatever their circumstances, as a judicial provision to ensure a fair and just trial.

2. The appointment of a mixed commission of judges, counsel, solicitors, and medical experts for the consideration of the question of dealing with inebriates who have been convicted of a criminal offence. This inquiry should have special reference to the best procedure to be pursued; whether, 1, if penal, by cumulative punishment or otherwise; or, 2, if curative, by medical treatment for a diseased condition, with due provisions for classification, occupation, hygienic measures, and elevating influences."

Another Danger in the Profession—Charged with Kissing.—An Irish physician was recently tried under the charge of attempting to kiss a young married woman while in his consulting room. Five thousand dollars represented the amount claimed for damages. The woman's story proved to be exceedingly incredible, and there was little doubt of its being the creation of an hysterical mind. The jury promptly found a verdict for the physician.

Treatment of Influenza.—Based upon his observations during the epidemic of one year ago, Dr. John William Moore, of Dublin, concludes as follows, touching treatment: "The treatment of the affection turns upon common-sense principles. It is expectant, palliative, and symptomatic. There is no specific for influenza; but the most useful drugs to employ in its treatment are, 1, quinine; 2, antipyrin (except in young children, and the weakly); 3, salicylate of sodium, especially in effervescence; 4, phenacetin; and 5, effervescing citrate of caffein."

MEDICAL ITEMS.

MISSOURI MEDICAL ASSOCIATION.—The date of meeting of this Association is given as May 19, 20 and 21—instead of May 12, as formerly announced—and the place of meeting is to be Excelsior Springs.

ETHER DRINKING IN MICHIGAN.—A correspondent communicates to *The Medical Age* the prevalence of one sort of ether drinking—the compound spirits of ether—in the northern counties of the State of Michigan, principally among the Finns, Swedes, and Poles. It is generally consumed with alcohol or whisky.

A RARE CASE.—Dr. Thomas D. Dunn of Westchester, Pa., reports (*University Medical Magazine*, May, 1891) a case of ligation of the common carotid artery, in a child three and one-half years of age, for hæmorrhage following peri-tonsillar abscess, and with the result of recovery of the patient. The case is remarkable, both from the disease, and the operation in one so young.

THE CHAIR OF THERAPEUTICS IN JEFFERSON MEDICAL COLLEGE.—Dr. Hobart Armory Hare was elected, April 22, 1891, by the Trustees of Jefferson Medical College, to fill the Chair of Materia Medica and Therapeutics. Dr. Hare is a young man, yet has already distinguished himself as an investigator and medical author, and will no doubt find scope for continued and added renown in the field of his new relationship.

STIMULUS FOR MEDICAL WORKERS.—Dr. Baillarger, the late eminent French alienist, has bequeathed to the French Academy of Medicine a sum sufficient to produce a biennial revenue of $400.00, which will be offered as a prize for the best work appearing—during the intervals between which it becomes available—on the subject of mental diseases.

A RESIGNATION.—It is announced that Prof. J. M. DaCosta has resigned the Chair of Practice of Medicine at Jefferson Medical College which he has occupied with such honor for upwards of nineteen years.

CASES IN OBSTETRICS.—An interesting tabulation of the first 1,000 cases of obstetrics in the Sloan Maternity Hospital, New York City, appears in the pages of the last issue of the *American Journal of Obstetrics*. The oldest patient confined was 46, while the youngest was 12 years and 10 months of age. The largest number (659) were between 20 and 30 years old. Also in the largest number (547) it was the first pregnancy, while in one case occurring in the table it was the patient's seventeenth confinement. Of the presentations 936 were vertex, and of these 610 occupied the L. O. A. position. As to operative interference, there were 112 in which it was required, as follows: induction of labor, 12; forceps, 83; version, 14; craniotomy, 3. Of twin cases there were 13, about one in seventy-seven of the whole number of women delivered. In the forceps cases the returns are certainly noteworthy. Out of the 83 cases none of the mothers died. Of the 84 children 75 were living, 9 still-born, including premature twins. In nearly all the cases the long curved forceps (McLane's) were used, both in high and low operations. Of the fatal cases in this report we have as causes chronic Bright's disease, 1; rupture of uterus, 1; placenta prævia, 1; placenta prævia with contracted pelvis, 1; eclampsia, 1; septicæmia, 1; total, 6. On analyzing these deaths it is seen that one was from chronic organic disease and not labor; in another the patient was moribund when taken from the ambulance; in a third—placenta prævia—there was much delay and great bleeding before assistance could be given. The septicæmia case was admitted while in the second stage of labor, and was in a very filthy condition. The showing, therefore, is highly creditable.

ETHER DRINKING IN IRELAND.—The statement lately put forth of the prevalence of ether drinking among certain classes in Ireland, has had confirmation before a British committee. The Rev. Dr. Carter, Rector of Cookstown, gave evidence that ether drinking was not a rarity, but quite a common practice. Children, even, were given to the habit, obtaining the intoxicant from beggar-women who tramped the country. On market days particularly a great number of people would keep themselves drunk at small expense, taking a given quantity of ether at certain intervals. Some Government action will probably be taken at an early date.

FUND FOR A BIOLOGICAL LABORATORY.—The trustees of Columbia College have determined to devote the late Mr. Charles M. DaCosta's bequest of $100,000 towards a laboratory of biology.

TOPICS OF THE WEEK.

THE CENTENARY OF THE MICROSCOPE.

It is generally thought that the invention of the microscope goes back to the close of the fifteenth century, or, to be more precise, to the year 1590, when, in the city of Middleburg, in Holland, two spectacle-makers, named Janssen, invented both the telescope and the microscope. This date for the invention, according to which its third centenary would arrive in 1890, does not rest on authentic documents, but is based on assertions published in 1665 by the physician, Peter Borel. He denied that Galileo, Drebbel and others deserved the credit of having invented the telescope; and in order to demonstrate that the invention of that instrument, as well as of the microscope, was due to the Janssens, produced some documents which showed that the two spectacle-makers, having invented the telescope in 1590, presented a specimen of it to Prince Maurice, Stadtholder of the Netherlands, and to the Archduke Albert. Later on, however, the telescope of Prince Maurice became a microscope, in a letter of William Borelli, who declared that he had always heard in Middleburg, his native city, that the Janssens had invented these optical instruments, and further that, when he was ambassador at London, in 1619, he had seen in the hands of Drebbel the identical microscope that the Janssens had presented to Prince Maurice.

Professor Govi, however, in a work which demonstrates the excellence of his judgment and his vast erudition, has collected a series of documents, which not only seem to restore the merit of the invention of the microscope to Galileo, but show the various vicissitudes of the discovery itself. The first hint of the transformation of the Holland telescope into a microscope is found in a little book published in 1610 by Wodderborn, a pupil of Galileo. Speaking of the wonderful qualities of the telescope, Wodderborn adds, in praise of Galileo, that "with the instrument could be perfectly distinguished the organs of motion and sensibility in the smallest animalcule," so that the particular formation of multiplied eyes in very small animals could be perceived. This new application of the telescope by himself Galileo did not deny, though he never directly affirmed it. In the National Library at Paris is preserved a letter by Canon Tarde, in which he speaks of visiting Galileo in Florence, in 1614, when the latter was sick in bed. Notwithstanding, to Tarde Galileo gave ample explanation of a microscope then in his possession.

Whether the invention of the simple microscope be due to Janssen or Galileo, to Drebbel is due the merit of having produced at Rome, in 1624, the compound microscope. The difference between the two hardly needs explanation. The simple microscope magnifies with a single lens, or with several lenses so close together that they act like a single lens. The compound microscope has two or more lenses, separated by a convenient distance from each other, and which act separately. In 1669, Eustachio Divini constructed a colossal microscope which magnified 140 times. A little after, Bonannus invented a horizontal microscope which magnified 300 times.

In the seventeenth century were laid the foundations of micrography, a science which, by the study of the minute anatomical elements and their functions, has made such great progress under the name of histology, and been such a fertile cause of important discoveries. With the microscope, Malpighi, by the minute examination of the tissues, confirmed the theories about them he had previously formed; Leuwenhœk discovered the globules of the blood and the structure of the nervous fibres; and Swammerdam dissected insects, of the most minute organs of which he gave descriptions still considered perfect.

In the eighteenth century, observes Henocque, but few modifications were made in the microscope. To mention all the improvements made in the instrument during our century would be tedious. During the last forty years enormous advances have been made in science by the aid of the microscope, of which the usefulness has been greatly increased by the skill with which the matter to be examined is prepared, and by the aid of photography. Micro-photography dates from 1840 only; but since that date it has had an uninterrupted series of noteworthy improvements.

Besides histology, created by the microscope, by which our acquaintance with the most hidden structure of organisms is constantly increasing, bacteriology, with its rapid succession of discoveries of the highest importance, owes its existence to the microscope. Those little beings, those microörganisms, which, by the change of the medium in which their evolution is effected, can produce so much good or so much evil, and of which it takes several millions to occupy the tenth part of half an inch in space, can now be identified according to their species, notwithstanding their changeable aspect. We can estimate the rapidity of multiplication, the number, the dimensions, and the singular manner in which by dividing themselves, or by means of a sort of buds or spores, the microörganisms reproduce themselves.

In the examination of the inorganic world the microscope has had results not less precious. The wonderful phenomena of crystallization, the exact form of the crystals, the more precise in proportion to their minuteness, the modifying properties of the light called forth by the thinnest layer of a mineral, the interior texture of rocks, all these can be studied with a precision impossible before the invention of the improved microscope. And, finally, not to mention all the triumphs achieved by the instrument, it has had an application which formerly would have seemed paradoxical, since the microscope has been employed to show the particulars of the nature of the surface of the planets, particulars which have been made clear by microscopic observations of instantaneous photographs.—Ernesto Mancini, *Nuova Antologia, Literary Digest.*

MEASURING THE PERCEPTION OF ODOR.

At a recent meeting of the French Academy, Secretary Berthelot exhibited a new instrument called the "olfactomèter." The inventor is M. Charles Henry. The object of the apparatus is to determine the minimum rate

of odoriferous vapor per cubic centimetre of atmospheric air perceptible to the human olfactory nerves. The olfactomèter consists principally of a graduated glass tube which moves within a paper envelope. The tube is held to the nose and the paper gradually withdrawn. As soon as the subject of the experiment perceives the odor of the material contained within the glass tube, the latter is withdrawn, and the quantity of vapor which has escaped is calculated from the known capacity of the tube, and the degree marked by the paper envelope. The cubic space affected by the odorous vapor is simultaneously determined by means of a small areometer. The inventor of the instrument shows that the perceptibility of different odors by different subjects varied enormously, the two limits of his experiments falling between 2 milligrams of ether per cubic centimetre, and one-thousandth of a milligram of oil of wintergreen per cubic centimetre.

FRIEDLANDER'S PNEUMOCOCCUS AS A FERMENT.

It has been known for some time that Friedländer's pneumococcus is capable of inducing fermentative changes in suitable solutions of glucose and cane sugar, this having been first discovered by Brieger. His observations have quite recently been confirmed by Dr. Percy Frankland, Mr. Arthur Stanley, and Mr. William Frew, who have just communicated a paper on the subject to the Chemical Society of London. They further found that the organism ferments maltose, milk sugar, raffinose, dextrin, and mannitol, but that, like the bacillus ethaceticus, it does not attack dulcitol. They made a special study of the fermentations of glucose and mannitol, determining quantitatively the proportions in which the several products are formed. These products are in each case ethyl, alcohol, acetic acid—generally accompanied by a little formic acid and a trace of succinic acid—carbon dioxide, and hydrogen. Both the glucose and mannitol were in all cases only partially fermented, and the decomposition of the glucose was especially incomplete, glucose being apparently less readily attacked by the organism than mannitol and cane sugar. The fermentation was not rendered more complete by furnishing the organism with a more abundant supply of nitrogenous food. The products of the mannitol fermentations were not only qualitatively similar to those obtained in the fermentation of the same substance by the bacillus ethaceticus, but the relative proportions in which they were formed were almost identical, the ratio corresponding closely to the molecular proportions 2 C_2H_5. OH : CH_3. COOH.—*The Lancet.*

THE POPULATION QUESTION IN FRANCE.

This subject naturally continues to exercise the minds of French statisticians and scientists. Dr. Jomileff discusses it in the February number of the *Archives de Tocologie*. The true cause of the stationary population appears undiscovered—perhaps the theory that the race is naturally not prolific, or that climate and habits are against fertility is correct. Levasseur, Dr. Jomileff notes, has recently shown that the question of professions and socio-religious ideas has little direct influence on fertility. Thus statistics make members of the liberal professions less prolific than farmers and business men, but the liberal professions best allow celibacy, or often enforce it, and late marriages are common. Social and religious ideas seem to have little influence, contrary to what might have been supposed. In strictly religious communities—Protestant as well as Catholic—a large family is held to be an honor, if not a blessing, and checks to impregnation are deprecated. Yet statistics show that the fertility of such communities is not higher than that of large bodies of men and women, chiefly laborers, where the religious element is weak and where checks are openly tolerated. Britanny, the Maritime Alps, and Corsica are fertile in children, and the population are strict in religious observances and almost free from corrupting influences; yet the inhabitants of the valley of the Garonne, also a strict and pious folk taken as a whole, have very few children. Lastly, some districts where the peasants are comfortably off, have a low fertility, others show an increasing population, whilst the same irregularity is seen in different departments, where the country people are poor and ill-fed.—*British Medical Journal.*

THE INFLUENCE OF EXERCISE ON DIGESTION.

Dr. Streng, in a lecture before the Medical Society of Giessen on the Influence of Exercise on the Digestion, which has been published in the *Deutsche Medicinische Wochenschrift*, states that he concludes from his own experiments that this influence is of a retarding nature. His experiments, however, suffer from the fact that he always injected 300 cubic centimetres of water before obtaining the contents of the stomach, so that the proportion between gastric juice and water continually varied. The first experiments in the clinic at Giessen were made on two dogs. Twenty-five grams of meat, suspended in 300 cubic centimetres of warm water, were twice injected into the fasting stomach, and after one feeding the dogs were compelled to remain for three hours in absolute bodily rest, while after the other feeding they were made to take active exercise. After the three hours the contents of the stomach were obtained and analyzed. The quantity did not essentially differ in the two cases; the experiments consequently tended to prove that exercise does not influence the time required for digestion. The chemical analysis also detected no difference. The same results were obtained by substituting the white of an egg for the meat. The experiments were then repeated twenty-five times on three men with healthy stomachs. Two of these suffered from sycosis, and the third from incipient muscular atrophy. They were fed each time with 200 grams of minced meat, a bun, a plate of bouillon, and three spoonfuls of mashed potatoes, and the contents of their stomachs were obtained four hours and a half afterwards. The exercise after meals consisted partly in gymnastics, partly in walking; absolute rest was obtained in bed. These experiments gave the same results as those on the dogs, the difference resulting from the chemical analysis being especially imperceptible. The author therefore concludes that the gastric function is in no way influenced either by muscular action or by absolute rest.—*The Lancet.*

PRACTICAL NOTES.

INFANTILE DIARRHŒA.

1. Withdraw all milk for from twenty-four to thirty-six hours. 2. Regulate the quantity and quality of the food and the frequency of giving it. 3. Give plenty of cool water. 4. Reduce the temperature with the bath. 5. Give medicines of an antiseptic and astringent character and stimulants as needed. 6. Wash out the colon two or three times a day.—*Archives of Pædiatrics.*

IODOFORM AND ARISTOL.

Dr. Richtmann recommends (*Nouveaux Rem.*) that aristol be used to replace iodoform, since it presents all the advantages of iodine and thymol without any of their disadvantages. Aristol does not cause irritation; its absorption is not followed by any phenomena of intoxication, and its odor is not disagreeable. Being less volatile than thymol, its use is especially indicated in extensive burns. It may be prescribed as a powder, or an ointment, or given in solution. The preparations used by Richtmann are as follows:

℞. Aristol pure, gm. 10.
S. For external use.

℞. Aristol, gm. 1.
Ether, gm. 10.
S. For external use.

℞. Aristol.
Ol ricini āā, gm. 1.
Collodion, gm. 8.
S. As a paint for the region affected.

℞. Aristol, gm. 2.
Paraffin ointment, gm. 18.
S. For local application.

℞. Aristol, 1 to 5 cgm.
Cocoa butter, q. s.
S. For urethral or vaginal bougies.

—*Medical Standard.*

SEDATIVE FOR BABIES.

Dr. Van Goidtsnoven, of Atlanta, gives a formula with which he has had most gratifying results in restlessness, spasms, deliria, and in all cases requiring a sedative, anodyne, anti-spasmodic or somnifacient.

℞. Camphor. monobromat., gr. xvi.
Ext. hyoscyami fl., gtt. xvi–xxx.
Syrup lactucarii (Aubergier's), f℥viij. ℥.
S. A tablespoonful every hour till relieved.

—*Dixie Doctor.*

PHENACETIN IN SCIATICA.

Sciatica is not only one of those affections which are extremely annoying and painful to the patient, but on account of its persistency often greatly tries the patience of the physician. At the clinic of Prof. Landon Carter Gray most benefit has perhaps been obtained from phenacetin, given, say, in tablets of four to eight grains every three or four hours. There are a good many cases, however, which do not respond to it very markedly. Doubtless, too, there are many cases of sciatica neuritis, rheumatism, gout, etc., in which a diagnosis of sciatica is erroneously made; but perhaps more frequently sciatica is mistaken for one of these affections.—*Practice.*

BISMUTH FOR ECZEMA OF INFANTS.

The following formulæ are given in *Nouveaux Remèdes:*

℞. Bismuth. subnit., ℥v.
Zinci oxidi, ℥iss.
Acidi carbolici, m. x.
Vaseline, ℥j. ℥.
To make an ointment.

In case there is much irritation paint on the following with a soft brush:

℞. Bismuth. subnit., gr. xl.
Glycerine, ℥ijss.
Acidi carbol., gtts. vj.
Aquæ rosæ, ℥iv. ℥.
To be well shaken.

RESORCIN IN ACNE.

Isaak recommends the following:

℞. Resorcin, ℥j to ijss.
Zinci oxidi pulv., āā ℥ijss.
Amyli pulv., āā ℥ijss.
Liquid vaseline, ℥v. ℥.

Rub on the part affected morning and night, or it may be used at night only, and removed in the morning with sweet oil.

ACUTE BRONCHITIS.

The citrate of potassium is a favorite remedy of Dr. H. C. Wood in acute bronchitis; his formula is, he says, the most reliable and efficient sedative cough mixture that he has ever used:

℞. Potass. citrat., ℥j.
Suc. limonis, f℥ij.
Syr. ipecac, f℥ss.
Syr. q. s. ad., f℥vj. ℥.
Sig. A tablespoonful four to six times a day.

Another favorite expectorant with this writer is oil of eucalyptus, which may be given in five minim capsules every three hours. It is only of use after expectoration is established.

MOUTH WASH.

David uses the following mixture as a tonic and antiseptic mouth wash (*Medical News*, February 21, 1891):

℞. Thymol, 7 grs.
Borax, 15 grs.
Water, 1½ ozs. ℥.

A few drops of this are to be placed in a wineglassful of warm water, and the mouth rinsed with it. In cases in which the breath is fetid, owing to deposits about the tonsils and gums, the following wash is said to be serviceable:

℞. Borate of sodium, 15 grs.
Alcohol, ½ drachm.
Water, 1 pint.
Thymol, 7 grs. ℥.

SOCIETY PROCEEDINGS.

Gynecological Society of Boston.

Annual (218th Regular) Meeting.

The Gynecological Society of Boston held its 218th regular meeting at the Medical Library on January 8, 1891, with the PRESIDENT, W. SYMINGTON BROWN, M.D., in the chair.

THE ANNUAL ADDRESS ON GYNECOLOGICAL PROGRESS

was delivered by the President, who said: Our third by-law requires that the retiring President shall deliver an Address upon the previous year's progress in gynecology,—a law, by the way, which, since my connection with the Society, has been rarely attended to. On the present occasion I propose to discuss very briefly a few recent suggestions by prominent gynecologists, principally relating to menstruation.

Dr. E. C. Gehrung, of St Louis, read a paper before the American Gynecological Society at its meeting in Boston, in September, 1889, on arrest of menstruation. He thinks that sanguineous menstruation is an abnormal process, an inherited disease, eventually to be got rid of through the Darwinian process of evolution. He says that "menstruation is not necessarily sanguineous." The practical part of the paper consists in a proposal to arrest menstruation in all cases where the loss of blood would be injurious to the patient. He asserts "that bloody menstruation, whether profuse or scanty, may be safely repressed (preferably by the vaginal tampon) whenever the individual cannot or should not spare the blood thus wasted, and that great benefits may be derived from this restriction in otherwise incurable or partly curable cases." Dr. Gehrung has practiced this method successfully for several years. The vagina is thoroughly tamponed, a new tampon being inserted every 48 or 60 hours. Another member said that he also had employed this method with great success.

Dr. Johnstone, of Danville, Ky., strenuously objected both to the theory and to the treatment. He believes that menstruation results from the erect posture, as proved by the fact that menstruation occurs in the higher apes, who stand erect, or nearly so, most of the time. Quadrupeds possess a rich plexus of lymph vessels connected with the womb; whereas the human uterus is almost destitute of such vessels. The proposed treatment did not seem to meet with much favor by those present at the meeting.

Dr. A. W. Parsons, of Northampton, Mass., proposes to do away with napkins during menstruation and substitute an antiseptic tampon, inserted through a glass speculum, as soon as the premonitory symptoms appear. This is removed when saturated and another roll inserted. Dr. P. claims that the tampon supports the congested uterus and is more cleanly. I have not learned that this suggestion has been generally adopted by women and scarcely expect that it will be practiced. One serious objection is that it necessarily keeps the vaginal walls apart, which naturally are in contact; and, judging from my own experience, it is a very difficult thing to induce patients to insert a cotton or wool tampon, even when it is needed to counteract prolapsus. They prefer to wear a pessary, which does not need to be removed for weeks or months. If used as a substitute for napkins, the tampon should consist of wool, with a covering of cotton, and a string attached to facilitate removal.

Dr. H. P. C. Wilson, of Baltimore, recommends the performance of laparotomy during menstruation. He refers to the paper of our Dr. H. R. Storer, read at the first meeting of the American Gynecological Society in 1876,[1] in which Dr. S. concludes "that for pelvic operations, all things being equal, it is better to select the week immediately following the cessation of the catamenia for all such operations." Dr. Wilson differs from this conclusion. He says, "for laparotomies involving the pelvic organs, my experience teaches me to select the uterine flood rather than the uterine ebb." This is the opinion of a surgeon who has had abundant opportunities to test it, and who is well known for conservative tendencies; and I think that the majority of gynecologists are now of the same opinion.

The question so frequently discussed during the last ten years, whether menstruation depends on ovulation or is independent of that process, has had some light thrown on it by a case recently reported in the *British Medical Journal* (Sept. 27, 1890). Dr. J. A. Robertson reports the case. The woman was 23 years of age. She commenced to menstruate before she was 14 years old, and continued to do so regularly for six years. During the following three years the quantity became less and less and the periods irregular, until Sept., 1887, when the flow stopped entirely. Pain, from the ovaries, extending to the top of the sacrum, kept constantly increasing; lately she spat up blood every day. Both ovaries were removed on Jan. 29, 1889, and the patient made a good recovery. She began to menstruate the following April, and continued to do so regularly until October. In June she was married, and was delivered of a boy August 13, 1890, which weighed about 10 pounds. The labor was protracted, forceps were used, and the child was still-born.

I think it is evident that a portion of one ovary must have been left, or supplementary ovarian tissue existed; and the fact that impregnation resulted weakens the Tait argument that menstruation is independent of ovulation. Dr. R. says:

[1] Dr. Storer's paper does not appear in the Society Transactions. It was afterwards published in the Edinburgh Med. Journal.

"I was not aware of leaving any ovarian tissue. Indeed, my aim was to extirpate the ovaries thoroughly, and I thought I had done so. I suspect, however, that a small portion of healthy ovarian tissue had reached up to or beyond the hilus of the right ovary, and that this may have taken on regular ovarian functions. This, of course, is merely conjecture."

Dr. L. C. Coe has an excellent paper in the *Medical Record* for August 9, 1890, on "The Dangers attendant upon Artificial Prolapsus Uteri." These are: overstretching of the ligaments; tearing of adhesions; starting inflammations; and causing an abscess to burst internally. I have no doubt that the rude way in which the uterus is often brought down to the vulvar orifice occasionally causes one or more of these mishaps. We are too much disposed to copy the doings of German hospital surgeons, who operate on a class of women widely different in constitution from the bulk of our American women. And while I do not say that artificial prolapsus should never be produced, I think that the seldomer it is had recourse to the better it will be for our patients.

The uterus normally is the most movable organ in the body. Practitioners sometimes forget this fact, and try to fix an organ by a pessary or otherwise, which nature intended should be allowed a great deal of freedom. And the practice of gynecology seems to change its position about as freely as the uterus itself does. No specialty I am acquainted with is so much subject to fashionable changes as ours is, keeping pace with the multiform revolutions which fashionable clothing inaugurates. But I need not waste your time by proving an assertion which I scarcely expect will be disputed.

Before closing allow me to call your attention to a few personal items. I believe that a greater inclination to look after constitutional measures than formerly has arisen in the profession. Dr. Field and the late Dr. Warner did more than the rest of us to bring about this result. I hope to see some of the younger members follow in their footsteps; for the constitutional field is not yet thoroughly gleaned.

Some progress has also been made in recording the after effects of surgical operations. Sir Spencer Wells inaugurated this reform, and has spent more time in perfecting it than any other surgeon. One of the most recent discoveries in this field is that a mild melancholia, generally of brief duration, is apt to follow surgical operation on the genital organs. I have seen one case recently, and several others have been reported to me.

So far as I am aware there has been only one death of an active member during the past year. Several members have resigned, but at least an equal number have joined our Society in 1890. I am pleased to see that a considerable number of the new members are young men, full of energy, believers in the virtue of cleanliness, some of whom will probably leave names behind them which the medical world will "not willingly let die."

If I might be allowed to give a hint to men much better qualified than myself, it would be this: Cultivate an *esprit du corps*. We are members of the oldest Gynecological Society in the world—not in itself a slight honor. Let us make up our minds to stand by one another, to take an interest in one another's success in consultation and otherwise; subject, of course, to the welfare of the patient, which is the first dogma in every good surgeon's creed. And I have no doubt, when the time comes for each of us to render an account of our stewardship, he or she will not be embarrassed by the reflection that "it is more blessed to give than to receive."

Tennessee State Medical Society.

Fifty-Eighth Annual Meeting held in Nashville, April 14, 15, and 16, 1891.

(*Concluded from page 644.*)

SECOND DAY—AFTERNOON SESSION.

DR. A. J. SWANEY, of Gallatin, contributed a paper on

RETAINED PLACENTA IN MISCARRIAGE; HOW SHALL WE TREAT SUCH CASES?

He said the dangers from retained placenta in miscarriage were hæmorrhage and septicæmia. When the delivery of the placenta is prolonged ought we still to abstain; ought we to wait or ought we to interfere actively in order to forestall these dangers which almost certainly will result and further interfere at a time when it is far easier than later to remove the placenta? Those who favor active interference are Tyler Smith, Murray, Simpson, Leishman, Mundé, Grandin, and many others. The reasons given for active interference are the frequency of these dangers in prolonged delivery of the placenta. Simpson directs that if the cervix is dilated or patent to act at once; if it is not dilated he dilates it at once. The woman is then anæsthetized, the uterus depressed as much as possible by the external hand and with the index finger of the other hand he removes the placenta and membrane. If he cannot sufficiently depress the uterus with the hand he does not hesitate to forcibly drag it down by a double tenaculum fixed in the cervix. Mundé and Grandin go still further and curette the cavity of the uterus with special instruments made for the loosening of adherent placenta and its removal from the uterus.

The authorities who counsel waiting for serious complications before interfering are just as many. We mention Ramsbotham, Davis, Burns,

Fleetwood Churchill, Grailey Hewitt, and many others. Active intervention does not mean unnecessary interference. Nature is ever to be given a chance, but when we see that her efforts are futile certainly it is but rational to assist her, and this should be done as directed by Mundé, by placing the woman in the left lateral position and with a dull wire curette remove the placenta or any part of the secundines that may remain through a Sims speculum. This is far better and easier than the method advised by Simpson, of dragging or pressing down the uterus and introducing the finger into the uterine cavity. The uterine cavity should then be washed out with hot water, slightly carbolized, through a Jameson's uterine douche, and this should be repeated every six or eight hours until all fetor disappears from the lochial discharge.

Hæmorrhage after miscarriage, even when we believe the placenta and secundines have been removed, invariably means retention of a part of the placenta or secundines, said the speaker. Profuse hæmorrhage may occur for weeks from this cause, and in such cases we should boldly explore the uterine cavity and remove any offending matter that may be present. In the first twelve weeks of pregnancy the dangers from hæmorrhage and septicæmia are not so great and the expectant plan is more justifiable. After the third month it is criminal negligence to wait and subject a woman to the dangers arising from retained placenta when she can be relieved by an operation, which, if properly done, can do no harm, and spare her the risk of hæmorrhage and septic poisoning. In short, the author believes the early removal of the secundines is easy and safe, and forthwith guarantees the woman against the dangers of hæmorrhage and sepsis.

DR. J. L. JONES, of Bells, read a paper entitled

INDIGO AS AN EMMENAGOGUE,

in which he said his attention was first directed to this drug as an emmenagogue in July, 1887, from an essay published in the *Medical and Surgical Reporter*, of Philadelphia, by Dr. S. L. Gount, of Lafayette, Ind. Acting on the suggestions offered by Dr. Gount, he had used it in many and various cases.

His first case was a young lady, 20 years of age, who had not menstruated in five months. He had been treating her for three months with the usual remedies without any effect, so he made up his mind to give indigo a trial, which he did. He ordered indigo ℥ij, subnitrate of bismuth ℥ss, well mixed. Of this the patient took one-half teaspoonful in one-third of a glass of water, three times daily, for nearly four weeks, when one day he was sent for in great haste. On his arrival he found the patient in bed, and comfortable. He was then told by the mother that her daughter, while walking in the garden, without pain or warning of any kind, began to flow. The gush was followed by a gentle flow which lasted only for a short time. In five days she was well, and has not suffered from amenorrhœa since. Dr. Jones has since used indigo in thirteen cases with but one failure, and this case proved to be pregnant.

During the administration of the drug the os uteri becomes soft and patulous, admitting the end of the index finger. There is often a serous discharge from the vagina. The urine becomes brownish green in color, and its odor is offensive. The stools are watery and offensive.

THIRD DAY—MORNING SESSION.

DR. J. A. WITHERSPOON, of Columbia, read a paper on

DIABETES,

in which he confined his remarks principally to diabetes mellitus. He said the term diabetes mellitus is a symptomatic one, being a deviation from health in which the processes of nutrition are seriously deranged, and presenting a group of complex symptoms, the most conspicuous of which are, first, frequent micturition, passing large amounts of pale, saccharine urine. Coincident with this is an insatiable thirst and dryness of the mouth and fauces, which is by far the most annoying symptom, the freest draughts of water giving but little or no satisfaction. The skin becomes dry and harsh, with complete absence of perspiration, and followed by a general pruritus, sometimes impossible to relieve. These were some of the many symptoms of the disease.

The etiology of the disease is yet an unsolved problem. It is an affection of adult life, but few cases being reported in children, and those always fatal. The only recovery reported, so far as the speaker was aware, was a girl 12 years of age.

The treatment is conveniently divided into dietetic, medicinal, and the hygienic. The dietetic is by far the most important. We must exclude from the bill of fare all food stuffs containing starch or sugar, for two reasons: 1. The normal action of the liver in its glycogenic function is seriously deranged and incapable of oxidizing the maltose sent to it by the digestion of carbo-hydrates, and they are therefore useless aliments. 2. They seem to aggravate and increase the glycosuria. A strict meat and green vegetable diet agrees better than any other. Some give milk, others forbid it entirely. He had found in the few cases he had treated that it was impossible to continue for long periods any one diet. The pure skim-milk diet is meeting with much favor. Alcohol, and all spirits, except small quantities of light wine or beer, must be forbidden. This regimen alone will lessen the quantity of sugar eliminated, but it is necessary to use in connection certain medical treatment. The drugs

giving the best satisfaction are opium, or its alkaloids, morphia or codeia; ergot, arsenic, and many others. Of all the drugs mentioned, codeia has been by far the most satisfactory in Dr. Witherspoon's hands. It was first suggested by Dr. Pavy, of London, and has the great advantage over its sister alkaloid, morphia, in not producing sleep. It is more efficient, less dangerous, and does not produce the troublesome constipation caused by morphia or opium. Next to it, the speaker would place ergot, for its physiological effect upon the blood-vessel walls. Recently sulphonal and antipyrin have been used with reported good results. He had never used antipyrin in this disease, for the reason that it is recommended in from 30 to 60 grain doses. He did try sulphonal in 10-grain doses, three times a day, and in two days it produced so much giddiness and sleepiness that he quit giving it. He had only used arsenic as an alterative tonic after the sugar had disappeared from the urine. With this treatment the patient should be advised to take light exercise, always short of fatigue, and their surroundings should be good and well ventilated. Alkaline carbonated waters are often of great utility.

TREATMENT OF STRICTURES OF THE MALE URETHRA.

This was the title of a paper read by DR. J. W. HANDLY, of Nashville.

He said strictures of large calibre, if they be recent, but poorly organized and of the linear variety, may be treated by dilatation, which must be continued for months. But should the stricture be densely fibrous, and not easily dilatable, the cutting operation becomes necessary, for which purpose he prefers the Otis improved dilating urethratome with which the surgeon can accurately divide any stricture to any size desired.

Strictures of small calibre, situated in advance of the bulbo-membranous junction, unless seen very early and found to be unusually soft and dilatable, furnish a typical condition for internal urethrotomy, that in which it is attended with the least possible danger and greatest prospect for a permanent cure. Should the contractions be so great that the Otis urethratome cannot be used, he had found Bank's whalebone dilators, which are made in four sizes, of great advantage in opening the canal so as to admit of the urethratome, and considered them very useful.

Strictures of small calibre posterior to the bulbo-membranous junction require a very different course of treatment, since internal urethrotomy at this point is often attended with profuse hæmorrhages, fever, rigors, or other disturbances equally as disagreeable. Strictures of this variety permeable only to filiform bougies, may be treated in one of the four following ways:

1. After the filiform has been introduced it may be allowed to remain *in situ* for two or three days and another passed alongside of it to serve as a guide for the introduction of a tunnelled sound, later to be followed by the ordinary soft or steel bougies. This is good and safe surgery in the absence of retention.

2. The surgeon may attempt to conduct a tunnelled sound over it at once, to be followed by gradual dilatation.

3. He may conduct over it a grooved staff and then proceed to the performance of external urethrotomy, or,

4. He may use the staff as a guide for the Massionneuve urethratome and may immediately perform internal urethrotomy.

OFFICERS FOR 1892.

The following officers were elected for the ensuing year:

President—Dr. J. W. Penn, Humboldt.

Vice-President for Middle Tennessee—Dr. J. A. Witherspoon, Columbia.

Vice-President for East Tennessee—Dr. C. E. Ristine, Knoxville.

Vice-President for West Tennessee—Dr. C. H. Lovelace, Dukedom.

Secretary—Dr. D. E. Nelson, Chattanooga.

Treasurer—Dr. J. P. C. Walker, Dyersburgh.

Next place of meeting, Knoxville, second Tuesday in April, 1892.

DOMESTIC CORRESPONDENCE.

Forced Respiration.

To the Editor:—In a recent number of THE JOURNAL I find an address by Dr. G. E. Fell, on "Forced Respiration." Allow me to draw your attention to an article which appeared in the year 1822, and which may possibly be of interest to the medical fraternity.

I am in possession of the *London Journal of Arts and Sciences*, Vol. 3, London, 1822, page 93: "Account of an apparatus for restoring the lost action of the lungs, in consequence of suspended animation from drowning, etc.; invented and communicated by John Murray, Esq."

There is, perhaps, no subject which ought to excite a deeper interest, than that of suspended animation. The number of human beings who are hurried suddenly out of existence, by being deprived of that necessary pabulum, atmospheric air, form annually a lamentable catalogue; and, although a Goodwin, a Hawes, a Cogan, and many others of the benevolent of our species, have done much in exciting attention to this interesting subject, yet it is to be lamented, that the scientific world has not paid so much attention to it, as its importance most unquestionably and imperiously demands.

FORSAN SCINTILLA LATEAT.

Plate 4, figs. 3, 4, and 5, exhibits drawings of an apparatus, which I have invented for restoring the lost action of the lungs. It consists of two cylinders concentric with each other; the inner one is three inches in

diameter, the exterior one four inches, forming a space of half an inch round the cylinder, which is to be occupied with water, heated to 98 degrees, the animal temperature to elevate the air contained in the interior cylinder to that degree.

The piston is solid, and moves horizontally; the piston rod is perforated, to receive a metallic pin, which, being introduced, shortens the stroke, checked, by the plate covering the end of the cylinder, and gives the means of apportioning the volume of air to the capacity of the lungs, which is to be determined by the victim of experiment being of tender age, or adult. This will obviate the danger of rupturing the lungs. To the pipe proceeding from this cylinder, is affixed a cell and cock, with an elastic tube terminating in a mouth-piece and plate of leather.

The stopcock is so constructed, that, when the handle is parallel with the pipe, as in the figure, there is a free communication established between the lungs and the cylinder, to the exclusion of external air. When on the other hand the cock is turned the quadrant of a circle, the communication with the lungs is cut off, and there is a free channel opened between the cylinder and the external atmospheric air.

The lateral cell, appended to the cock, will be found of varied use and importance. Should the subject of experiment have been the victim of carbonic acid gas (choak damp), a drop or two of ammonia may be mingled with the air in the cylinder, and when thrown into the lungs will condense the mephitic gas. If a septic poison—as sulphuretted or arsenicated hydrogen—have occasioned the asphyxia, a few drops of solution of chlorine or nitromuriatic acid will destroy the septic virus. Should the atmosphere be too dry, a small portion of water put into the cell, will mix with the air, and impart additional elasticity; and if we desire an additional stimulus, a drop or two of ether posited here, will expand in the air of the cylinder, and this mixed atmosphere will act with all the consequence of nitrous oxide. These provisions for various reasons, are valuable auxiliaries in returning respiration.

When the machine is to be used, the victim of suspended animation is to be raised in a gently inclined position opposite to the operator; the nostrils are to be plugged up, and the plate of leather fixed on the mouth, as nearly air-tight, by means of white of egg, etc., as possible; this must be kept in its position by means of a ribbon tied round the head. The operator over against the victim, manages uniformly and equally the piston. The apparatus being adjusted in the manner described, the air is first withdrawn from the lungs, and then ejected laterally; and the piston rod being drawn to the extremity of the cylinder—if an adult—the pure atmospheric air fills the instrument; the communication with the lungs being restored by turning the stopcock parallel with the pipe, the operator begins. About twenty plunges of the piston a minute may be the proper number. It will not be necessary to change the included air until natural respiration is restored; because, until this does take place, the blood cannot eject its excess of carbon, the consequence of the circulation of that fluid; but when this is required, it is instantly accomplished in the manner already described.

This invention has met with the most unequivocal testimony of approbation from several eminent physicians and surgeons, and gentlemen of distinguished mechanical genius.

The apparatus, with my latest improvements on it, of which a drawing accompanies this paper, is formed of Britannia metal, and has been presented by me to the Royal Humane Society. It is made to rest in a groove, which may be easily fixed to a table by means of a clamp. It possesses an attached pin, which being fixed into the perforated piston rod, readily apportions the injected air to the capacity of the lungs, whether of an adult or child. Hence there can be no danger of injuring the lungs. The flexible tube should be formed of caoutchouc, and be perfectly elastic and pliant like the bougies.

In this improved apparatus, the heated water—which will be better at some degrees above 98 to compensate for the loss of temperature sustained by the conducting of the metal and the transit of the air through the pipe into the receptacles of the lungs—is supplied exterior to the cylinders and above; thus affording every facility of ascertaining the temperature by a thermometer. There is also an exit pipe, with a stopcock to permit the water when cooled, to flow, so that without altering the position of the apparatus in the least, it may be supplied with heated water anew. The valve which communicates with the lungs, is Mr. Trevithick's conical one, used in the steam engine. The internal cylinder, being one foot long and three inches in diameter, will contain 8,482 cubic inches of air; about half this extent will be sufficient for an ordinary inspiration.

A. W. HERZOG, M.D.

Hoboken, N. J.

MISCELLANY.

LETTERS RECEIVED.

Aberdeen, S. Dak., Aberdeen Daily News Co.
Albuquerque, N. M., Dr. J. P. Kaster.
Bellefontaine, O., Dr. W. W. Hamer.
Boston, E. L. Damrell.
Brooklyn, N. Y., Dr. R. M. Wyckoff, Dr. Albert Gihon, Dr. L. D. Mason, Dr. F. C. Raynor.
Chambersburg, Pa., Dr. R. W. Ramsey.
Chicago, Dr. L. L. McArthur, Dr. L. H. Mettler.
Chilhowee, Mo., Dr. C. T. Sweeney.
Cincinnati, O., J. Taft.
Coquille City, Ore., Dr. W. C. Owen.
Corinna, Me., Dr. O. H. Merrill.
Dallas, Tex., Dr. D. Green.
Denver, Col., Dr. C. R. Crandall.
Detroit, Mich., Citizens' Savings Bank, Dr. E. L. Shurly.
Eagle Grove, Ia., Dr. C. H. Morse.
Elmore, Wis., Dr. William Hausmann.
Florence, Ala., Dr. E. L. Jones.
Galveston, Tex., Dr. Geo. Dock.
Indianapolis, Ind., Dr. E. S. Elder.
Jersey City, N. J., Dr. E. Vockvoth.
Leigh, Neb., Dr. J. C. McKinley.
Lincoln, Neb., Dr. F. S. Billings.
Livingston, Mont., Dr. W. H. Campbell.
Lodi Cal., Mrs. J. L. Sargent.
London, Eng., Kegan Paul, Trench, Trübner & Co.
Montgomery, Vt., Dr. B. W. Davis.
Nashua, N. H., Londonderry Lithia Spring Water Co.
New York City, Reed & Carnrick, A. L. Chatterton & Co., Chas. H. Phillips Chemical Co., The Drevet Mfg. Co., J. Walter Thompson, J. H. Bates, Dr. Jas. Moran, W. P. Cleary, Geo. P. Rowell & Co., F. H. Drake & Co.
Panora, Ill., Dr. C. O. Sones.
Paris, Tex., Dr. Wm. M. Moore.
Philadelphia, Dr. R. J. Dunglison, Dr. J. H. C. Simes, Dr. A. L. Hummel, N. W. Ayer & Son.
Robertsdale, Pa., Dr. W. H. Johnson.

Official List of Changes in the Stations and Duties of Officers Serving in the Medical Department, U. S. Army, from April 25, 1891, to May 1, 1891.

Capt. George E. Bushnell, Asst. Surgeon U. S. A. (Camp Pilot Butte, Wyo.), is granted leave of absence for twenty-one days, to take effect on or about May 1 next. Par. 9, S. O. 49, Dept. Platte, Omaha, Neb., April 20, 1891.

Lieut.-Col. James C. McKee, Surgeon, having been found incapacitated for active service by an Army Retiring Board, is relieved from further duty as attending surgeon and examiner of recruits at Philadelphia, Pa., and will proceed to his home and report by letter to the Adjutant General of the Army. By direction of the Secretary of War. Par. 3, S. O. 96, A. G. O., Washington, April 28, 1891.

Official List of Changes in the Medical Corps of the U. S. Navy, for the Week Ending May 2, 1891.

Asst. Surgeon George A. Long, granted two months' leave of absence.

Medical Inspector A. A. Hoehling, detached from Navy Yard, League Island, and waiting orders.

Surgeon W. H. Jones, ordered to Navy Yard, League Island.

P. A. Surgeon O. D. Norton, detached from Naval Hospital, Chelsea, Mass, and waiting orders.

P. A. Surgeon F. J. B. Cordeiro, ordered to Naval Hospital, Chelsea, Mass.